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DOE'S MAGNETIC FUSION PROGRAM

HEARING

BEFORE THE

SUBCOMMITTEE ON
ENERGY RESEARCH AND DEVELOPMENT

OF THE

COMMITTEE ON
ENERGY AND NATURAL RESOURCES
UNITED STATES SENATE

ONE HUNDRED FIRST CONGRESS

FIRST SESSION

ON THE

DEPARTMENT OF ENERGY'S MAGNETIC FUSION RESEARCH AND DEVELOPMENT AND DEMONSTRATION

JUNE 14, 1989

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DOE'S MAGNETIC FUSION PROGRAM

WEDNESDAY, JUNE 14, 1989

U.S. SENATE,
SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT,
COMMITTEE ON ENERGY AND NATURAL RESOURCES,
Washington, DC.

The subcommittee met, pursuant to notice, at 2 p.m., in room SD-366, Dirksen Senate Office Building, Hon. J. Bennett Johnston (chairman) presiding.

OPENING STATEMENT OF HON. J. BENNETT JOHNSTON, U.S. SENATOR FROM LOUISIANA

The CHAIRMAN. The hearing will come to order.

Today, the Subcommittee on Energy Research and Development is holding an oversight hearing on the Department of Energy's program of research, development and demonstration for the production of energy from thermonuclear fusion. The DOE fusion program is at a crossroads, and we have a lot to discuss today. However, one of the things we will not be discussing is the recent controversy—the confusion—over cold fusion. The question of whether researchers at the University of Utah have seen fusion in a jar is a very technical, very interesting question that is being talked about all over the scientific community. And we may discuss that on another day at another time, but not today.

The question of the future of DOE's existing magnetic and inertial confinement fusion research program belongs here and today. Not that we know all the answers. We very much need the advice of the community of scientists, engineers and managers who are specialists in the fusion science field. But it is up to us to hear that advice and to decide.

That is the spirit in which we should proceed in this hearing. We are looking for advice on the future of this program. Let me say where I think the problems are that need your attention so that we can address them.

The President's fiscal year 1990 request for magnetic fusion R&D is $349 million. The President has requested $169 million for inertial confinement fusion, a program that as now constituted really has more to do with weapons development than energy development. So the fusion program absorbs a significant number of science dollars at a time when available science dollars are too few to meet the needs of science.

The President's fiscal year 1990 request for magnetic fusion is more than the request for nuclear fission R&D and more than the request for clean coal technology. It is three times the request for

renewable energy R&D and four times the request for energy conservation R&D. It is more than half the request for all of high energy physics and more than the fiscal year 1990 request for the superconducting supercollider.

The National Academy of Sciences study that Dan Dreyfus will report on today concluded that the present program's goals cannot be achieved within the current budget. The NAS study says that to achieve current program goals, the budget will have to be increased by 20 percent, held steady for five years, and then increased again by 25 percent. These kinds of increases are simply not on the budget horizon.

We will need to spend the dollars we can get for fusion wisely. We need to configure the fusion program so that its goals can be achieved within the dollars that are likely to be provided.

The next big expenditure planned in the magnetic fusion program is the Compact Ignition Tokamak, the CIT. With both phases, the cost of this facility approaches $1 billion. In addition, we are talking with the Soviets, the Japanese, and the Europeans about an International Thermonuclear Experimental Reactor, the ITER. This would be a multibillion dollar facility. With these proposed expenditures in front of us, we need to be very sure we know where we are going and at what rate we are planning to get there.

[Prepared statements of Senators Ford and McClure follow:]

STATEMENT OF SENATOR WENDELL H. FORD

Hearing on Fusion Energy Research,
Development, and Demonstration

June 14, 1989

Today the Subcommittee on Energy Research and Development is meeting to receive a status report on the Department of Energy program of research on fusion. We will also hear proposals from Dr. Robert O. Hunter, Jr., Director of Energy Research, for the redirection of the Department of Energy's fusion program. These proposals have been talked about a great deal, but as far as I know, there has been no public discussion of them. We hope to have such a discussion today. There is a distinguished panel of witnesses who can represent the views of the fusion community on these proposals.

I hope the record of this hearing can be useful in arriving at a consensus about what to do about the fusion program. This is potentially a major energy source with strong environmental benefits. We would be irresponsible not to develop and option for fusion power. The question is not whether to develop the option, but how to develop it.

I commend Dr. Hunter for his efforts to deal with a very difficult budget situation and a very complex set of scientific and engineering questions. He is trying to make this program work. We need to work together with DOE and the fusion community to see that the program does work.

OPENING STATEMENT
SENATOR JAMES A. McCLURE
COMMITTEE ON ENERGY AND NATURAL RESOURCES
WEDNESDAY, JUNE 14, 1989 (2:00 P.M.)

FUSION ENERGY

Good afternoon. I am pleased that the Subcommittee has scheduled this hearing to examine the Department of Energy's fusion program and its FY90 budget request for fusion energy research. I am particularly pleased that we had the wisdom and pure common sense to resist scheduling this hearing during the time when "cold" fusion was the "hot" topic of discussion. Because we waited patiently through that media event, we now have the luxury to devote our attention more productively towards the real policies and programs that will be guiding our fusion research into the twenty-first century.

As is often typical of the Department's budget proposals -- and, for that matter, their testimony in support of their proposed budgets and their budget justifications -- reveal very little of the policy goals or programmatic objectives that underlie all the fancy text and tables. What I have heard in briefings and by second and third hand knowledge is much more meaningful and, in some instances, quite contradictory to what is suggested by the formal text. Therefore, I am anxious to hear what the Department REALLY proposes to do with its FY90 fusion program and, even more importantly, what it envisions as the proper direction for that program to

take in the foreseeable future.

Ever since Admiral Watkins took office as Secretary of Energy, I have been encouraged by the many initiatives that the Secretary has undertaken to rethink, reorganize, and refocus some of the Department's entrenched programs and policies. This is a healthy sign. But it must be carried out with care and thoughtful due process. It must be done with input from all knowledgeable parties. It must withstand the scrutiny of peer review.

Furthermore, any changes brought about by these initiatives must be supported by the community that it affects and supportable by the science and physics. Change for the sake of change is not good enough, and I doubt that this is what Admiral Watkins intends. He seems to me to be a very thorough, reasoned leader. He is definitely goal-oriented. Yet he would probably be the first to admit that he has been inundated with crises, top priority policy decisions, and personnel and management decisions that would overwhelm any ordinary person.

This all leads up to the points I would like to make today with respect to the status of the Department's fusion energy research programs. I am familiar with the present program, facilities, and policy. I am aware of a recent re-examination of the direction and content of these programs, especially the intent to delay the Compact Ignition Tokamak (CIT) construction while the Department pursues additional experiments to better understand the physics that necessarily underlies the design and ultimate

success of any dramatic scale-up. I am sympathetic to this school of thought, yet nowhere in the scientific community do I see the suggestion of any withdrawal of support and commitment to the CIT as the logical next step in fusion technology. So I therefore must conclude that, while a certain slowdown and refocusing is justifiable, an abandonment of the CIT is simply NOT RATIONAL. I hope the Department will reinforce my reading of the situation.

In recent days there has been introduced a totally new wrinkle in the program, the proposal to reduce our investment in magnetic fusion, to augment our commitment to inertial confinement fusion, and then to somehow compete the two concepts against each other in the selection of a technology for achievement of fusion ignition. This represents a sudden shift that I do not fully understand. I also am not sure that it is a necessary or advisable shift. And I question whether this shift in the program's goals and objectives won't set-back the Department's fusion program.

I hope that the Department and the other witnesses before us today will enlighten me on this new twist, whether or not it makes any sense at this time. Once again, I look forward to the information this hearing will hopefully bring to light.

The CHAIRMAN. I understand that the Director of Energy Research, Dr. Hunter, has some proposals to address the pressures that are operating on the program. I also understand that these proposals are controversial. Controversial or not, we need to get them on the table. This program is very unlikely to get more money. Therefore, we will need to provide new direction for it. We need to get on with providing that direction, and the man to give us that direction is Dr. Hunter.

STATEMENT OF DR. ROBERT O. HUNTER, JR., DIRECTOR, OFFICE OF ENERGY RESEARCH, DEPARTMENT OF ENERGY

Dr. HUNTER. Thank you, Mr. Chairman.

I would like to make some brief opening remarks and then, of course, would be pleased to respond to any questions that you may have.

There now exists in the Department's fusion program a considerable amount of controversy over the scope and design of future fusion programs. The immediate focus of controversy within the magnetic fusion program is construction of the Compact Ignition Tokamak, the CIT. Later testimony in this session will present some of the technical analysis of CIT performance and arguments concerning initiation of the construction project.

The Department is persuaded by the findings of the Magnetic Fusion Advisory Committee subpanel 22 that the CIT has a very low probability of achieving its ignition objectives. The Department is also convinced, both by MFAC 22 and by extensive discourse with members of the magnetic fusion community, that the fundamental physics of tokamak confinement is not understood.

Secretary Watkins is considering deferring the CIT construction project until we understand enough of the fundamental phenomena controlling tokamak transport to assure ourselves that it will succeed. The Department does not believe that the CIT is worth constructing if there is not a high probability that it will ignite.

Secretary Watkins has taken a strong personal interest in the future of fusion R&D. He has outlined the essential principles of a comprehensive new policy for fusion energy that embraces both magnetic and inertial confinement fusion and has directed me to develop a detailed policy upon these principles.

The intent of the policy is to revitalize both fusion programs, focus them on the key unknowns, and push both to achievement of ignition of fusion reactions in new experimental devices within about 10 years. This will permit us, early in the next century, to evaluate the practical potential of both approaches based upon solid experimental evidence and to choose the best integration of technologies to advance to commercialization in a fusion reactor.

The CHAIRMAN. Dr. Hunter, let me stop you at that point. I have read your statement, and why do we not have a little give and take here on your position?

Now, you state that it will take three to four years of highly concentrated research to be able to answer the remaining physics questions about CIT, that CIT as presently proposed and with the present state of the research on physics has a very low probability of reaching break even. Is that correct?

Dr. HUNTER. Of reaching ignition, sir.

The CHAIRMAN. I mean ignition.

Dr. HUNTER. Yes, sir.

The CHAIRMAN. Which means that it would really be a mistake, as you see it now, to go into a $1 billion program, overselling it to ourselves, with a little likelihood that it would produce what it is supposed to produce.

Dr. HUNTER. Yes, sir. We have pursued—and I think you will hear the testimony later here today—we have pursued vigorously in the last several months trying to understand this very point; and we do not believe that you should start to construct the machine unless you are confident that you have a high chance of ignition and of building it for the dollars and on the schedule that you state at the outset.

The CHAIRMAN. Let me state, Dr. Hunter, we are certainly willing to entertain here or elsewhere contrary opinions, and while we have a very, very high respect for you, we do not refuse to listen to the other side. But if this is correct that there is a low probability of ignition and more or less an overselling of the program, I would urge those who are strong proponents of CIT to recognize that that is the right thing to do; because the days of willy-nilly expenditure on fusion or any other scientific expenditure in this Congress, I think those days are over with. We do not have the dollars we used to have; there is not the naivete about, you know, just spending whatever without having a pretty high degree of confidence. You can see that in everything from the Star Wars program to research and development on energy.

We have got to demand a higher degree of predictability and a higher degree of success than we had in the past. And so I think it is the right thing to delay that program. A billion bucks is a billion bucks, and we are just not going to get into these programs without having some high degree of confidence.

Now, what is your view about inertial confinement fusion?

Dr. HUNTER. Well, the view of interial confinement fusion I would say rests on two bases: one, I think that there is some remaining unknown physics in that, but we have reason to believe that we can get a target in the laboratory to ignite and burn with high gain, and at a reasonable amount of energy in the drivers that we use—lasers, ion beams and what not—and energy on the order of 10 megajules.

The second part is the cost to build that driver. And we, today, have not demonstrated the driver technology that would let you build that facility for substantially under $1 billion. So we propose a parallel path with the magnetic fusion program where we spend three to four years in the ICF area concentrating on the physics unknown and on technologically demonstrating a driver, of the various candidates—and there are several that we would pursue—at a low enough cost and at a high enough level of confidence in the cost that we could build an ignition facility, an ICF, by the turn of the century as well.

So you really have a dual track competition here. The status in each case is not exactly the same. In the CIT, the technology, what it is going to cost for the magnets, and the various things to power

it, I think, are well understood, and the confinement physics is a major unknown.

The CHAIRMAN. Well, let me state here, Dr. Hunter, on first blush, on the proposal to put money over in inertial confinement fusion: First of all, the Department of Energy had all but abandoned inertial confinement fusion as an energy program; and when we thought about doing that here in the Committee and in the Energy and Water Subcommittee of Appropriations, DOE said well, it has got important defense uses. And we understand that, and we have gone forward in the defense proposal.

But here, all of a sudden we find that it has gotten a new life, and we are going to create a new program. Is this a different program from the defense program?

Dr. HUNTER. It incorporates elements of the defense program but, in addition, it is to provide some additional funds because that program today, as you pointed out in your opening comments, is running at roughly half the magnetic fusion level.

The CHAIRMAN. I mean, where would it be? At Livermore?

Dr. HUNTER. We do not have a site selected. In the competitions that we would undertake for this facility, it would be a competitive site selection.

The CHAIRMAN. I mean, would it be the same program that you have for defense or a new program?

Dr. HUNTER. No. There would be a new program, and it would concentrate on the driver technology to show that you can build a low cost driver at good scale.

The CHAIRMAN. Well, where does this new-found confidence and enthusiasm for this technology come from given—I will not say abandonment—but virtual abandonment by DOE?

Dr. HUNTER. Well, where it comes from is very substantial physics progress, some in classified arenas; but there has been very substantial physics progress in the last decade. And whereas in, say, 1980 the amount of energy you might require to drive a target and get high gain burn was still varying—I have heard numbers in my career as low as 100 joules to a kilojoule to as high as 10 to the 9th joules—by conducting these tests on both the coupling of the driver energy to the target, as well as the implosions of the targets themselves over the last decade, I think we now have a much higher degree of confidence and certainty that we do know the numbers that are required roughly to make ignition in the laboratory take place. So I think there has been a lot of progress in the physics in the ICF program.

The CHAIRMAN. Well, frankly, over a period of years we have felt that yes, there ought to be a competition, but that that competition needed to have been run a long time ago and effectively had been run and the choice made. I mean, here we are talking about rejuvenating new programs. And, frankly, I do not know where the dollars are for this.

You probably saw what the House did to magnetic fusion. What they did is cut $68 million out of the budget for magnetic fusion. And you are talking about starting a new program. I mean, I do not know where these dollars are going to come from. We are trying to get the dollars, too, for the superconducting supercollider, and the House did not come up with much money for that. They

cut $50 million from the President's budget. They gave $200 million to the superconducting supercollider, and that is something that I, at least, have a lot of enthusiasm for.

You are not asking for that inertial confinement money in this budget, so there will be time to deal with it. But I think it is fair to let you and others know that we have got to have a very strong case made to go to any dual track. And you look at the best of worlds, and it takes so long—I mean, we are talking for commercialization in 2050, or something like that; and that is even beyond the time that I am in the Senate. Not by much, I hope.

Dr. HUNTER. Sir, I can certainly assure you that it is beyond the time I may well be in my position, but let me make a couple of comments there.

One is that the goal of this program is really strongly focused, and it is for the country to know if fusion is a real energy alternative, which we think is very valuable to us, we can find out around the turn of the century. And this dual track gives us the highest chance of finding out. That is why this program is structured the way it is.

And the other principle that we have been working through—it is not easy—is we want to try to do it without any major increase in the funds now existing in these two programs. So it will involve an adjustment inside the programs.

And we will not try to pursue an enormously broad-based, very aggressive in both engineering and technology program during the course of this. We will focus on getting ignition, so then we will know if this thing is really there as an energy option. And that is why the Secretary and I believe this to be an extremely valuable program. I think that, in the last 10 years, the physics and the technology have come a long way, and it warrants doing this now.

The CHAIRMAN. Well, what kind of a funding profile are you talking about for inertial confinement fusion?

Dr. HUNTER. The funding profiles that we have worked through in our internal planning—and it has not yet come through formally, and we will have it reviewed—are to transfer $50 million from the MFE account into another account, a civilian ICF account, to put into driver development in fiscal year 1990, and in fiscal year 1991 to add an additional $50 million that we would obtain from the rest of the programs in the Department. We are going through that in the 1991 budget exercise now.

The CHAIRMAN. You said $50 million in fiscal year 1990 and an additional $50 million in 1991 or 1990?

Dr. HUNTER. Yes, sir. In 1991. And then those funding streams, which will be $300 million for magnetic fusion and roughly $260 million for ICF, would continue. We would need that funding stream through the end of the decade in today's dollars to get ignition.

The CHAIRMAN. On inertial confinement?

Dr. HUNTER. And on magnetic confinement.

The CHAIRMAN. $300 million each?

Dr. HUNTER. $300 million in magnetic fusion and $260 million, $250 million in ICF.

The CHAIRMAN. And when does ICF grow from $50 million?

Dr. HUNTER. ICF, $50 million in 1990 and an additional $50 million, going to $100 million, directed towards this decision in 1992 in 1991.

The CHAIRMAN. All right. Well, I do not think you are going to get that kind of money. As I say, the House has already cut $68 million off of magnetic fusion and without adding in the inertial confinement. So a very, very strong case must be made, because we are competing among a lot of——

Dr. HUNTER. We understand, sir. And the program is at a crossroads. That is why last fall when I came in, and the Secretary, when he came in through his confirmation hearings, felt strongly about this program, that we do face a decision here. You have invested a lot of money and time. The program has promise and may conceivably be of great utility to us. On the other hand, we are under severe budgetary pressures.

And so what we would ask for is your consideration. We will be moving forward in the next one to two months to have this program reviewed carefully by an independent review body; and we would ask your consideration in being able to do this, even though it is an inconvenience and on very short notice.

The CHAIRMAN. Yes, we will be glad to do that.

Dr. HUNTER. Okay.

The CHAIRMAN. Senator Burns, do you have any questions?

Senator BURNS. I have no questions.

The CHAIRMAN. Thank you very much, Dr. Hunter. We will be in close touch with you.

First of all, before you leave, what would be the impact of this proposal on ITER?

Dr. HUNTER. ITER is an international collaboration. There is no impact of this proposal on ITER. In fact, I think there would be, in a direct sense, a very positive impact in that the confinement physics for the CIT ignition is essentially identifical to the confinement physics you need to have a good quantitative basis for ITER.

So I think you are very strongly contributing to, you know, a major unknown in that program. So I think it would be, on the whole, very helpful to that; and I think we ought to keep the ITER and international programs there as useful adjuncts in terms of developing, say, reactor engineering that we could not afford to do on our own.

The CHAIRMAN. Thank you very much, Dr. Hunter.

Senator McClure, did you have a word for Dr. Hunter before he leaves?

Senator MCCLURE. No. Thank you.

The CHAIRMAN. Thank you very much.

Dr. HUNTER. Thank you, sir.

[The prepared statement of Dr. Hunter follows:]

Statement of Robert O. Hunter, Jr.

Director

Office of Energy Research

Department of Energy

before the

Subcommittee on Energy Research and Development

of the

Committee on Energy and Natural Resources

United States Senate

June 14, 1989

Mr. Chairman and Members of the Subcommittee:

I am pleased to be here today to discuss the FY 1990 budget request for the Magnetic Fusion Energy program. The continuing goal of the Magnetic Fusion Energy program is to establish the scientific and technological base required for an assessment of the feasibility of magnetic fusion as an energy source.

Fusion as a source of energy is attractive for several reasons. The fusion fuels (deuterium and tritium) are easily obtained and virtually inexhaustible. In addition, fusion reactors can be designed to yield very little long-lived radioactive waste. Finally, fusion powerplants would have outstanding safety features because there are only small amounts of relatively low-hazard fuel present at any one time and the fusion reaction could not run out of control.

The greatest challenge in producing fusion energy for practical use is to develop a device that can heat the deuterium-tritium fuel to the required high temperature and confine the heated fuel long enough so that more energy is released in the fusion reactions than is used to heat the fuel. The Tokamak Fusion Test Reactor (TFTR) goal is to confine the fuel at high temperatures long enough to achieve "breakeven" condition--where the fusion energy released is equal to that required to heat the fuel. Beyond breakeven, the next major step in the fusion program will be to achieve ignition, the condition in which the fusion reaction products take over the heating of the plasma and the plasma begins to burn without external heat sources. Once ignition has been achieved, the next goal will be a long pulse, or steady-state, burn and technology development.

The program called for by the Department's strategy, and further defined in the Magnetic Fusion Program Plan of 1985, includes work on each of four key technical issues: the properties of burning plasmas, magnetic confinement systems, fusion materials, and fusion nuclear technology. Although the Fusion program supports work aimed at addressing each of the key technical issues, the FY 1990 budget request gives highest priority to the first two. This will include strengthened experimental and theoretical efforts in high leverage areas of research on tokamak confinement and burning plasma physics. Improved understanding of tokamak confinement and burning plasma physics will help to advance the program more rapidly and is expected to reduce the cost of the development of fusion energy.

Let me take a moment to comment on the four key technical issues. The first of these concerns the properties of burning plasmas. No experimental facility exists anywhere in the world to investigate burning plasmas. The present Compact Ignition Tokamak (CIT) proposal is a two-phase experimental attempt to attain ignition in a modest-size device and to study and optimize fully ignited plasmas, should they be attained. The proposed device makes use of equipment and facilities at Princeton Plasma Physics Laboratory to reduce the cost of CIT. The existing design, which uses only existing TFTR power supplies, does not assure ignition. The probability of ignition, based on empirical global scaling laws for confinement, is reasonable, particularly with the ability to add additional power supplies if warranted. I have requested that a review be conducted to examine the prospects for ignition, including the need for additional power supplies. We will provide the results of this review, including implications for the budget, in the near future.

All the qualitative features known to improve confinement (high magnetic field, high current, high plasma density, discharge shaping, etc.) are exploited in the CIT device. There is, therefore, a consensus throughout the fusion community that a device of this general type should be built. Efforts are underway to increase the ultimate capability of the device and to better specify the need for any additional power supplies or heating systems. Thus, the general parameter region for the device is known, and necessary R&D on magnet design is proceeding. The FY 1990 budget request, which I will discuss in a moment, includes funds for both R&D and initiation of construction activities.

The second key technical issue concerns magnetic confinement systems. Significant progress continues to be made in understanding the processes involved in confinement. During the past year, improved confinement in tokamaks has been extended. New confinement conditions were achieved in TFTR at Princeton Plasma Physics Laboratory and DIII-D at General Atomics. Additional confinement improvements were observed in the Joint European Torus (JET), ASDEX in West Germany, and JT-60 in Japan (up to a factor of 3) compared to previous results. Nevertheless, there is a pressing need to improve our understanding of the physics mechanisms controlling energy confinement. To improve our ability to produce, heat, and control the requisite plasmas, the FY 1990 budget request provides for the application of recently developed experimental and theoretical capability to the physics mechanisms controlling energy confinement because this information is critical for planning for CIT and could reduce the size and cost of future steps in fusion development.

A review is underway on the value of the physics performed on the various machines, as well as the relevance of the tokamak class devices to improving our knowledge of tokamak confinement. The results will be reported as they are available.

The third key issue concerns the identification and testing of materials for fusion systems. Achievement of the program goal will require development of new materials to reduce the induced radioactivity and, thereby, enhance the economic and environmental potential of fusion. Materials research is being pursued through cooperative international agreements that provide significant foreign contributions toward the operation of U.S. research facilities for mutual benefit. The FY 1990 budget request supports the core research program needed for U.S. participation in this international effort.

Advances in the basic engineering sciences will be required to address the fourth key technical issue, the nuclear technology of fusion systems. For this issue to be resolved, fuel-producing blankets must be integrated into other fusion systems and tested in a nuclear environment. These fusion blankets perform several functions, including converting the energy released by a fusion reaction into useful energy, creating the tritium fuel for the reactor, and allowing for recovery of this fuel, as well as shielding critical components from radiation. The FY 1990 budget request supports selected basic technology research that underlies the development of blankets for a fusion engineering test reactor.

Research carried out by the U.S. Magnetic Fusion program on the four key technical issues constitutes the basis for our participation in the world fusion program. This participation continues a long history of scientific

exchange and cooperation. The most significant current international cooperative effort is that aimed at developing a single conceptual design for an International Thermonuclear Experimental Reactor (ITER). The ITER device would, if built, be the follow-on to CIT that allows for the study of sustained ignition and burn physics. This device would also be used for the technology development necessary to lay the foundation for the design and construction of future devices. This effort, carried out under International Atomic Energy Agency auspices, includes the U.S., the USSR, Japan, and the European Community. After preliminary work and negotiation in 1987, the joint design effort began on April 21, 1988. The FY 1990 budget request provides for continuing U.S. participation in the conceptual design and validating R&D at the level of $16 million dollars per year per party. Therefore, over the three years of the ITER effort, approximately $192 million of effort will be devoted by the four parties to the preliminary design and supporting research and development. Once the design is complete, each participating country will have the information needed to make its own decisions regarding a future ITER-type facility.

FY 1990 MAGNETIC FUSION ENERGY BUDGET REQUEST

The FY 1990 budget request for Magnetic Fusion Energy is $349.2 million. This request includes $307.9 million for Operating Expenses, $13.2 million for Capital Equipment, and $28.1 million for Construction (Table 1). This budget request provides for high priority research based on increased use of existing tokamaks, application of additional diagnostic equipment to existing devices, increased efforts on development of a more attractive heating technique, and exploitation of innovative ideas for extending improved confinement.

Table 1

Magnetic Fusion Energy

($ in millions)

	FY 1988	FY 1989	FY 1990 Request
Operating Expenses	$295.7	$316.7	$307.9
Capital Equipment	18.7	21.6	13.2
Construction	16.9	12.4	28.1
Total	$331.3	$350.7	$349.2

Let me now summarize the Operating Expenses request for each of the interrelated functional areas within the Magnetic Fusion Energy program: Confinement Systems, Applied Plasma Physics, Development and Technology, Planning and Projects, and Program Direction (Table 2).

The Confinement Systems subprogram, with an FY 1990 Operating Expenses request of $168.1 million, addresses the two high priority technical issues for the Fusion program--improving magnetic confinement systems and determining the properties of burning plasmas. This research is focused on tokamaks and involves developing the data base needed to prepare for a burning plasma experiment and to identify an improved confinement system. The primary scientific issues being addressed by this research are energy confinement, heating, plasma stability, current control, and physics and particle control at the edge of a plasma.

In FY 1990, research will be conducted on several toroidal devices to investigate these physics issues and to prepare for performing the burning plasma physics experiments on TFTR and CIT. The confinement of high temperature plasmas will be studied on TFTR at the Princeton Plasma Physics Laboratory. Preparation for operation with deuterium-tritium fuel will be delayed to allow continued operation of TFTR to focus experiments on confinement in the world's hottest tokamak. To aid in the study of confinement physics, improved diagnostics will be installed for these experiments. The Operating Expenses request also includes $15 million for R&D related to the CIT project.

Table 2

Magnetic Fusion Energy

Budget Authority

($ in millions)

	FY 1988	FY 1989	FY 1990 Request
Operating Expenses			
Confinement Systems	$159.6	$174.8	$168.1
Applied Plasma Physics	74.7	78.2	73.2
Development and Technology	55.9	54.1	56.7
Planning and Projects	0.9	5.0	4.9
Program Direction	4.6	4.6	5.0
Subtotal	$295.7	$316.7	$307.9
Capital Equipment	18.7	21.6	13.2
Construction			
Compact Ignition Tokamak	8.0	3.5	5.5
General Plant Projects	8.9	8.9	9.3
Confinement Physics Research Facility	0	0	13.3
Subtotal	$ 16.9	$ 12.4	$ 28.1
Total	$331.3	$350.7	$349.2

A number of medium-sized toroidal devices will be operated to help improve our understanding of how to produce, heat, and control the plasma required for fusion energy. Scientists at the DIII-D device at General Atomics have made steady progress in determining the upper limits of a parameter called "beta," which is the ratio of plasma energy to magnetic energy. A world record beta of 8% has been achieved. Scientists at DIII-D will continue to explore alternate plasma shapes and operating modes predicted to increase beta, while also extending understanding of confinement obtained through magnetic field shaping.

Fabrication of the Alcator-C Modification at the Massachusetts Institute of Technology (MIT) will be completed in FY 1990. This experiment will test various aspects of the CIT design, physics assumptions, and projected operating modes. The Microwave Tokamak Experiment (MTX), at Lawrence Livermore National Laboratory, which was fabricated using the old Alcator-C device from MIT, began operating in FY 1989. It provides the unique capability to test the application of pulsed high power microwaves as a more efficient heating technique in tokamak devices.

Two devices are currently operating to study advanced toroidal issues. These devices are the Advanced Toroidal Facility (ATF) at the Oak Ridge National Laboratory and the Princeton Beta Experiment (PBX) at the Princeton Plasma Physics Laboratory (PPPL). These devices are conducting experiments to determine the existence of a second stability regime and determine the plasma physics requirements for operating with high performance and improved stability. The Advanced Toroidal Facility, which is a stellarator type device, is also looking at the issues of stability, transport, heating and particle control in this type of device. As the program continues to focus on

tokamak confinement physics in FY 1990, operation of ATF will continue, but on a half-time basis. The PBX facility, although valuable in the long run, will be mothballed in FY 1990 while resources at PPPL are focused on TFTR and CIT.

International collaboration will be relied on to carry out research on a number of related plasma issues in foreign facilities, including TEXTOR and ASDEX in West Germany, Tore Supra in France, the JET in Europe, and JT-60 in Japan. In each of these cases, the U.S. provides technical personnel and specialized equipment in return for participation in the planning and conduct of the research and sharing of the results.

The Applied Plasma Physics (APP) subprogram, with a FY 1990 budget request of $73.2 million, plays a major role in addressing the magnetic confinement and burning plasma issues at a very fundamental level. The subprogram conducts research on basic magnetic confinement physics, develops information on new diagnostic techniques and techniques to heat and control plasma, and provides computational support for the overall program.

In FY 1990, APP will continue to expand our understanding of magnetically confined plasmas through experiments with alternative magnetic configurations, including tokamaks, compact toroids, and reversed field configuration devices. These configurations provide different physical environments in which magnetically confined plasmas obey the same overall physics laws governing plasma stability as well as transport of energy and particles within the plasma. The Texas Experimental Tokamak (TEXT) at the University of Texas provides a center for investigating the basic physics of the transport of particles and energy within plasma and for developing diagnostics. Other, small, university plasma devices test new ideas on current drive and heating

or test specific theoretical predictions. Research with non-tokamak configurations includes: (1) design of a major reversed field device, the first concept to be tested in the Confinement Physics Research Facility at the los Alamos National Laboratory; (2) operation of a new reversed field device at the University of Wisconsin to investigate edge physics issues in reversed field configurations; (3) completion and first operation of a field reversed device at Spectra Technology with supporting work at the University of Washington; and (4) spheromak physics research at the University of Maryland and at the University of California, Berkeley.

In FY 1990, theoretical research will refine predictions of alpha particle physics in burning plasma, continue to develop predictive capability for radio frequency heating, and emphasize predictions and modeling of processes controlling the transport of energy and particles in toroidal plasma. Large scale computational needs are provided through the operation of the Energy Sciences computing network.

The Development and Technology subprogram, with a budget request of $56.7 million, develops components and systems for use in near-term and mid-term fusion experiments, and conducts research and development on long-term generic materials and fusion nuclear technology. This subprogram also performs conceptual design studies for future fusion devices. These studies identify component and system development needs that must be met to prepare for the eventual application of fusion. The international collaborative program, ITER, is funded from this subprogram.

In FY 1990, this subprogram will continue to pursue research on technologies needed to form, contain, heat and sustain a reacting fusion plasma.

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Development of higher field capability superconducting magnets will be continued to help reduce the plasma physics needs and reduce the size and cost of fusion reactors. The heating and fueling systems developed to date have enabled the production of record plasma conditions in fusion devices, and this U.S. technology is valued highly in the development of agreements for collaborative work internationally. Fueling the plasma with pellet injectors, which are funded from this program, is instrumental in achieving density profiles in plasmas which produce better confinement. The heating systems developed in the technology program are being used to achieve temperatures in present devices and are being improved to do the same in larger fusion devices.

Materials research will continue to develop low-activation materials that will reduce the need for long-term waste disposal and limit materials damage due to the bombardment of neutrons and plasma inside the fusion reactor. In this regard, Oak Ridge National Laboratory achieved significant progress in 1988 in the development of a reduced activation structural alloy for the ITER design. Technologies needed for various blanket concepts will also be investigated for their ability to perform the multiple functions of heat extraction, tritium production, and radiation shielding. Finally, design and R&D work will continue in support of ITER and in particular on the power handling and particle control issues.

The Planning and Projects subprogram primarily provides support for construction of major new facilities and fabrication of auxiliary components for these facilities. The FY 1990 Operating Expenses request of $4.9 million is primarily for compliance with requirements associated with the Small Business Innovation Research program.

The Operating Expenses request of $5.0 million for Program Direction provides for personnel and other costs associated with continuation of 62 full-time equivalent staff.

The FY 1990 Capital Equipment request of $13.2 million will provide for essential hardware to support the overall program. This equipment includes diagnostic and computer equipment, power supplies, and other components for experimental facilities.

The FY 1990 Construction request of $28.1 million provides $13.3 million for the Confinement Physics Research Facility (CPRF) at Los Alamos National Laboratory. CPRF is being proposed as a line-item construction project for the first time in FY 1990. The project was funded as an Operating Expense project through FY 1989. With a total estimated cost (TEC) of $75.6 million and an estimated completion date of FY 1993, the CPRF will bring to the program an experimental capability to explore the physics properties of a reversed field toroidal confinement experiment that has the theoretical capability, in a future device, of heating the plasma to ignition with lower magnetic fields. The FY 1990 request will support fabrication of the coils and vessel/shell.

The FY 1990 Construction request also includes $5.5 million for the Compact Ignition Tokamak. The project, with a TEC of $455.0 million, began in FY 1988 and is scheduled for completion in 1996. FY 1990 activity in support of the CIT will include detailed design of CIT systems and conventional facilities, long lead procurements for tokamak components, and initiation of site preparations.

The remainder of the Construction request, $9.3 million, is for General Plant Projects (GPP). These GPP projects at Los Alamos National Laboratory, Princeton Plasma Physics Laboratory, and Oak Ridge National Laboratory provide for the continuing minor alterations and modifications necessary to protect the government's investment in its facilities.

That concludes my statement. I would be happy to answer questions.

The CHAIRMAN. Next we have a panel with Dr. David Baldwin, Director of the Institute for Fusion Studies at the University of Texas, and Dr. Kim Molvig, Associate Professor for Nuclear Energy, Plasma Fusion Center at MIT.

Dr. Baldwin and Dr. Molvig, welcome to the committee. Your full statements will be put in the record, and we would prefer that you summarize the main points so that we can have a little give and take. But you may proceed as you wish.

STATEMENT OF DR. KIM MOLVIG, ASSOCIATE PROFESSOR OF NUCLEAR ENGINEERING, MASSACHUSETTS INSTITUTE OF TECHNOLOGY

Dr. MOLVIG. Senator Johnston, members of the committee, let me briefly introduce myself to you and my reasons for being here before you today.

I am a Professor of Nuclear Engineering at MIT. I was extensively involved in fusion research from 1976 to 1983, but have been virtually disconnected from the program for the past five years. It was partly for these reasons that I was asked to chair this recent MFAC subpanel 22, which essentially reviewed the program.

I have submitted lengthier written testimony. As you said, that will be transcribed into the record. I would like to work more informally from this attachment, which I have asked Ben Cooper to pass around to you. This has a few brief figures on it and a couple points that I would like to talk to individually.

Our panel, MFAC 22, made an extensive assessment of the physics of tokamak confinement, including lengthy technical sections about where we stood, and it made recommendations as to what could be done to improve that understanding. We also made some programmatic recommendations in that report, which is available for your reading.

I want to confine my comments here today, focus on the single issue of CIT. This is, of course, to be the centerpiece of the U.S. fusion program, and it should be the culmination of all we know on the program, science and physics understanding.

Now, let me speak first from the second page of what I had handed out to you, which is verbatim, quoted extracts from the panel report. And I want to stress before going through these—I will make a few comments about them—these are the consensus statements not only of the panel, but of MFAC, the larger committee which presides over it, and to a large extent the research community as well.

With regard to phase 1 of CIT—this is the proposal currently before Congress—the statement is, "Because of the level of performance required and the lack of additional heating power, the overall expectation for ignition in the first phase is very low."

Phase 2, which is not in the existing proposal, but it is contemplated as part of the project plan, involves more power supplies and additional auxiliary heating. The panel's statement on this is that "The CIT would have a high probability for ignition in the second phase with only modest enhancements—less than a factor of 2—over empirical scaling required."

Now, I want to depart from the written document here for a second to bring your attention to the word "empirical," because what this means is that our present understanding, the way we must design machines, is based on empirical knowledge, not a physical understanding. The mechanisms dominating energy——

The CHAIRMAN. Well, first of all, tell us what is the difference between phase 1 and phase 2?

Dr. MOLVIG. Phase 1 is the narrowly defined 440 form, whatever it is, proposal that you now have.

The CHAIRMAN. I do not know what a 440 form is.

Dr. MOLVIG. Construction Project Data Sheet. It has a total project cost of $580 million. It does not include the power supplies needed to go to higher fields and currents or the additional auxiliary——

The CHAIRMAN. Is phase 1 a machine and phase 2 improvements to the machine?

Dr. MOLVIG. In essence, yes.

The CHAIRMAN. What is the difference between phase 1 and phase 2 machines?

Dr. MOLVIG. Phase 1 has a lower capability and is expected to have a lower confinement performance.

The CHAIRMAN. Is phase 1 a designed machine or concept?

Dr. MOLVIG. It is pretty well designed. The design is being worked on. It has been a evolving concept, but it is a design, yes.

The CHAIRMAN. Now, as I understand it, the design, you think, gets within 90 percent of ignition, and then you lose your neutrons for some phenomenon you do not understand.

Dr. MOLVIG. Yes. The difficulty in the process is that in terms of our empirical understanding, the loss process is from the plasma, and the heating power provided by the alpha particles when fusion occurs scale in almost exactly the same way with plasma parameters. And if a machine is constructed where the constants in front do not work out—that is to say, if the alpha heating power fails to exceed the plasma loss rate—there is very little you can do in terms of changing plasma parameters to make the machine work.

The CHAIRMAN. It is the alpha particles that provide your heat, not neutrons.

Dr. MOLVIG. That is right. Yes.

The CHAIRMAN. Okay. And as I understand it, the phenomenon—you are confident you could get ignition, but you would have to put so much additional magnetic power on there that it would clearly be uneconomical.

Dr. MOLVIG. That is correct.

The CHAIRMAN. So the challenge is not just to achieve ignition but to do so at reasonable cost parameters that would make the machine useful. Am I correct?

Dr. MOLVIG. That is correct, Senator. We do know enough about this to realize that if you make a magnetic bottle that is big enough, it will certainly ignite.

The challenge for fusion in general for the economic future, and particularly with CIT, is to do it in a more clever way at smaller size and cost. And that is, in fact, the intent of the CIT.

The CHAIRMAN. And there is something that you do not understand about the physics of it that you think in three or four years you can learn?

Dr. MOLVIG. That is right.

The CHAIRMAN. So that then, in effect, you would not build your phase 1 machine; you would go directly to a phase 2 machine?

Dr. MOLVIG. Exactly.

The CHAIRMAN. All right. Let me hear from Dr. Baldwin on this, and we can go back and forth and the two of you jump into this.

STATEMENT OF DR. DAVID E. BALDWIN, DIRECTOR, INSTITUTE FOR FUSION STUDIES, UNIVERSITY OF TEXAS

Dr. BALDWIN. What was the question?

The CHAIRMAN. The question is does the fusion community agree with Dr. Molvig, and if not, why not?

Dr. BALDWIN. Let me give a little word of explanation why there are two phases, because it is part of the answer to the question.

There is a certain range of expectations—it is sort of a factor of two level—in the performance of the machine. The first phase of the machine is designed to ignite, if the optimistic end of that range pertains. If we are able to get the enhanced performance we have seen in the past, then it would ignite. It would do so at the power and current levels which are built into that phase, and we would have ignition at that point.

If, however, we begin to fall short of the best performance we have seen today, then we will need more power and more current. And that is, in a sense, the pessimistic end of the range of the expectation. And the facility is designed to handle the extra power and current which is necessary to ignite at that point.

So the flat-out statement that there is low expectation of ignition in the first phase is the reverse of the statement that you have to do everything just right to make it work. If we could do it that way, it would be very good news for fusion.

The assessment that it has a high probability of ignition in the second phase when you have got the facility operating at its fully rated capacity is a statement that we have accounted—we have allowed in the design for that eventuality, and therefore, there is a high confidence at that stage. So there is this range of expectations.

The CHAIRMAN. Is phase 2 just more power?

Dr. BALDWIN. It is just more power and more current. More heating power and more current in the same machine.

The CHAIRMAN. Well, really, what we want to do is we want to achieve it at phase 1, because if we know we can do it at phase 2, that means you do it at this unacceptable cost level, in effect, am I correct in that?

Dr. BALDWIN. If it really took the full capability, it would be a disappointment for fusion. I suspect what will happen is that the answer will fall somewhere in between.

The CHAIRMAN. I mean, is this a little bit like an airplane engine, phase 1 of which is at an acceptable weight; then the question is whether the propeller will go around at an acceptable

weight? And phase 2 weighs a thousand tons, and you know that that will make the propeller go, but the plane will not fly?

Dr. BALDWIN. No. I said that these were in the range of a factor of two in performance; they were not in the kind of numbers that you—that were invoked——

The CHAIRMAN. A factor of two is a hundred times, is it not?

Dr. BALDWIN. No, no. Two times. Two times in the performance range.

The CHAIRMAN. Oh, I see. I was thinking of orders of magnitude.

Dr. BALDWIN. Yes, indeed.

The CHAIRMAN. It just costs twice as much as we can afford.

Dr. BALDWIN. The cost for the additional heating, which I think is what you are getting at, is about an extra 50 percent in round numbers, yes.

The CHAIRMAN. All right. Dr. Molvig, you wanted to jump in?

Dr. MOLVIG. Yes. I want to make a comment, Senator. Thank you.

We do sometimes talk about a factor of two being our level of understanding of the empirical laws. I think it is important to realize that we need to go over a factor of 10 in machine performance and to be able to hit that target point to within better than a factor of two.

Dr. BALDWIN. That is correct.

Dr. MOLVIG. And this is the cause of my statement about the uncertainty, as we do need acute accuracy in predicting this target point, and that the outcome of the experiment is critically dependent, extremely sensitively dependent on getting that target point right. That is a consequence of our present empirical understanding.

The CHAIRMAN. Are not your alpha losses somewhat proportional to your power increase on your past experiments?

Dr. MOLVIG. Yes. What tends to happen when you try to heat the plasma to get larger alpha power, which has been the goal of the program for some time, is that the losses from the plasma increase in a somewhat depressingly fast way. And these two balance so closely that you have little flexibility in simply raising the temperature. You cannot contrive to make the overall experiment work a priori with the proper configuration and so on. And that is why the outcome of the experiment, the success or failure in terms of ignition or not ignition, is acutely sensitive to the physics parameters performance that is obtained.

The CHAIRMAN. Dr. Baldwin, do you think—as I understand Dr. Molvig, he is saying we do not understand the physics well enough. I mean, you can go out and put this experiment together which has a low probability of working, and it might work. But do you agree with him that three to four years of work on the physics would give us a higher degree of expectability that the thing would achieve—that phase 1 would achieve ignition?

Dr. BALDWIN. Yes, I do, indeed. Let me make a comment about this physics question, speaking as a theorist, which Dr. Molvig is as well.

The theory situation regarding this transport question is actually fairly good in the following two senses. First, there is a lot which is qualitatively well described by our theories, and in fact, I suspect

that in the idealized world of our calculations, we actually have identified the right mechanisms for this energy loss. The problem is recognizing it in the plasma.

In my written testimony I made the very rough analogy that it may be possible to identify individual instruments, but it is very hard to recognize them when they play them in the orchestra. And the next physics step we have to be able to make in this area is to recognize these mechanisms inside the plasma with the right diagnostics that we now have available.

Therefore, I see us poised, in a sense, to really be able to attack this problem. We have well-developed theories. We have well-developed computational methods for mapping these idealized models to the real experimental situations. And we have very sophisticated diagnostics to measure physical quantities deep inside the plasma.

The CHAIRMAN. Gentlemen, you are both principally, as I understand it, experts on magnetic fusion. However, let me ask you to rate your enthusiasm for inertial confinement fusion.

Dr. Molvig?

Dr. MOLVIG. Well, I would respectfully decline comment on the technical side of inertial fusion. I certainly welcome the competition. I think that will invigorate the research program and put pressure on the existing magnetic fusion people to get these answers. And I do echo Dr. Baldwin's sentiments that the ability is there in the program to answer these questions in two to four years.

The CHAIRMAN. Suppose we had to cannibalize magnetic fusion to do it?

Dr. MOLVIG. Well, that certainly makes it more difficult, but you have not presented us with an alternative, Senator.

The CHAIRMAN. Dr. Baldwin, what do you think?

Dr. BALDWIN. Well, I have a couple of comments to make on the plan. I have to say the plan is very new to me, so I am not reflecting the MFAC position in this regard. The plan is only newly revealed.

Two comments. One, we have talked these last few minutes about the question, the importance of doing the transport problem in the magnetic fusion.

The CHAIRMAN. The transport problem, you mean the loss of——

Dr. BALDWIN. The loss of energy, yes. And you just put your finger on the first difficulty with this revised plan is that it takes way the resources which are necessary to attack just that problem; so that it makes it very difficult for us to focus on that in a way that we would otherwise be able to do.

Second, on the question of a competition or shootout in 10 years' time, I am very concerned that the two devices which would be chosen for this comparison are neither—neither one of them are prototypical of the reactor that they were intended to represent. Both are physics—are devices specially aimed at getting at the physics of ignition in the respective programs.

The CIT is a compact machine; that is in the name. It is a high field. It is aimed at getting at physics questions in the smallest size. It is a little harder to say what would be the ICF ignition machine, because it is only a vision at the moment. But in all likelihood it would be single pulse. It would not be the energy gain that would

be necessary. It would be an energy gain, in fact, in the range of 10 rather than the 100 which is needed.

And so both of these machines would still have their questions at the end of the decade, because they would neither—neither of them would be prototypical of the reactor they were intending to represent.

The CHAIRMAN. Senator McClure, any questions?

Senator MCCLURE. I am a little bit unsure that I understand what I have heard. If I understand, the answer to the questions is there is a range of possibility with respect to success in the first phase. I think exactly how you assess that, there may be a difference of opinion, but there is no difference of opinion on that.

Dr. MOLVIG. That is correct.

Senator MCCLURE. Secondly, that the second phase implies the same machine, more power.

Dr. MOLVIG. That is right.

Senator MCCLURE. No disagreement on that?

Dr. MOLVIG. No.

Senator MCCLURE. Then what is the reason for delay to redesign the first phase, if all you are doing in the second phase is adding power?

Dr. MOLVIG. If I could answer, Senator, I think there are two answers to that. A physics understanding or experiment that was designed with a physics base rather than an empirical one may look a little different.

Senator MCCLURE. Then you might redesign the first phase?

Dr. MOLVIG. That is right.

Senator MCCLURE. So that if you had more understanding, you might build the first phase differently, and therefore, the second phase is not just simply adding more power.

Dr. MOLVIG. That is correct.

Senator MCCLURE. Dr. Baldwin.

Dr. BALDWIN. There are two features, two aspects, which go into the design of a machine like this. One is the magnets, the vacuum chamber, the kind of hardware, which is the long lead time item that takes time to construct. And the construction time for that part of it is, depending on the funding rate, is four to six years perhaps.

The second aspect is how you heat it and how you use it, and decisions on that do not have to be made today. The heating systems can be built at a later stage and take advantage of what we learn in between.

And so the strategy which has been recommended by the MFAC is one that I think I can best report simply by quoting two or three lines from their last forwarding letter, dated June 7. "We believe that progress in the remaining transport questions will be adequate within the next few years to provide a basis for ignition; that the CIT design, R&D and prototyping should proceed in parallel with the transport work; and that a physics checkpoint on the transport issue should then release construction funds for the project."

So this is a way to proceed in an orderly way to getting ready in the engineering sense for the project at the same time that we are

getting ready in the physics sense, with a very important decision point down the road in roughly 1992.

Senator MCCLURE. So what you are, in effect, suggesting is we not launch either phase 1 or phase 2, but continue to do some developmental research.

Dr. BALDWIN. The program plan at the moment does not have the actual cutting of metal, as they say, the actual construction of the machine starting until—I do not know the actual schedule, but around 1992 to 1993. So to some extent, this is only a question of interpreting as to whether you are proceeding with this R&D or not.

Senator MCCLURE. Well, let me ask this question of both of you. What little I know about it leads me to believe that there is a consensus that CIT is the next logical step, and neither of you are suggesting we change that. It is just the kind of work in the immediate future and the pace of that work towards construction is all you are suggesting.

Dr. MOLVIG. Yes. This is a point I wanted to make anyway, Senator. There is really no disagreement on the technical assessment, not large ones. It is how you translate this uncertainty in the physics into a programmatic recommendation. And there are those that would like to build the experiment right away, aggressively in spite of this uncertainty, but the Department's position, which I have supported, of course, is that we need to insist on a firmer physics understanding before going ahead with these very expensive facilities. And that is really the difference.

Senator MCCLURE. Dr. Baldwin, any——

Dr. BALDWIN. The MFAC statement I read to you emphasized the importance of doing the transport. And I remind you that there is also important R&D in a new engineering project of this sort. You cannot simply stop the development of these very high field magnets and the new technology—the fairly new technologies that are required for a machine of this type. So the MFAC recommendation, in effect, takes account of that engineering side as well as the physics side and said keep fires burning, with this important decision to be made very early in the next decade.

Senator MCCLURE. I have no further questions.

The CHAIRMAN. Gentlemen, thank you very much. We appreciate your testimony.

Dr. MOLVIG. Thank you, Senator.

[The prepared statements of Dr. Molvig and Dr. Baldwin follow:]

Testimony of Dr. Kim Molvig

Associate Professor of Nuclear Engineering

Massachusetts Institute of Technology

Chairman, MFAC Panel 22

before

The Subcommittee on Energy Research and Development

of the

Committee on Energy and Natural Resources

United States Senate

for

a Hearing on

The Department of Energy's

Magnetic Fusion Energy Research

Development and Demonstration Program

June 14, 1989

Senator Johnston, I want to thank you for giving me the opportunity to speak to your committee today during its hearings on the U.S. fusion program. Let me briefly introduce myself to you and my reasons for being here before you today.

I am an Associate Professor of Nuclear Engineering at MIT. By training and research interests, I am a theoretical physicist. My specialty has been plasma physics. I was very active in the fusion research effort between 1976 and 1983. During that time, I made research contributions to nearly every facet of tokamak confinement theory on subjects ranging from neoclassical transport to runaway electrons to high-frequency electromagnetic radiation phenomena to macroscopic tearing mode behavior to drift wave turbulence. One of my best recognized contributions concerned the turbulence processes that are thought to cause the energy loss from tokamaks. So I have a fairly detailed technical knowledge of the subject about which we are speaking today. However, for the past five years I have not been involved in fusion research.

Partly as a result of my experiences in the field and partly because I am not currently actively involved, Dr. Hunter asked me to chair the subpanel on Confinement and Machine Physics of the Magnetic Fusion Advisory Committee (MFAC).

My sub-Panel was asked to review the status of our understanding of tokamak confinement and to recommend actions that could be taken by the U.S. Fusion Program to improve this understanding. This was one outgrowth of the Transport Initiative and its associated reprogramming actions of which I am sure you are familiar. Let begin with some of excerpts from Dr. Hunter's charge letter explaining the motivation for this review.

> The issue of confinement, or energy transport, and how it scales from today's experiments to reactor-grade plasmas is a fundamental one for fusion. For tokamaks, this issue has come to the fore this year as the ITER team worked to specify a design point and, to a lesser extent, as we designed CIT.
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> For ITER, the assumptions made regarding confinement are key to setting the size and, therefore, the cost of the device. In this era of highly constrained budgets, a better understanding of tokamak confinement is necessary: to minimize costly margin in machine design and, ultimately, to improve confinement through control of the phenomena driving energy transport.
>
> It is, therefore, the policy of this office to give highest priority in the MFE program to developing a predictive understanding of confinement in tokamaks. I believe this is also consistent with the sentiments of the fusion community, as expressed through MFAC's 1988 summer study. To implement this policy expeditiously, we are initiating a number of changes in the MFE program. These changes include seeking new ideas for experiments, diagnostics, and theory, especially from scientists not now involved in the fusion program; providing additional operating time on selected tokamaks; and controlling transport in tokamaks.

The initial motivation for the transport initiative came from the International Tokamak Experimental Reactor (ITER), not the Compact Ignition Tokamak (CIT), for which Dr. Hunter had previously expressed strong support on several occasions. However, it became clear to me shortly after our panel began its investigation that the situation with CIT was far from clear. As a result, Dr. Hunter added an additional charge for the panel on Feb. 3, 1989 to consider the question of the likelihood of obtaining ignition in the CIT experiment.

The assessment of confinement made by the panel focused on the empirical understanding that the very substantial progress in tokamak confinement has given us, and the question of how confident we can be in extrapolating to next generation devices from such an empirical base. This is also the key question on which the success of the CIT experiment hinges. The empirical understanding takes the form of global scaling laws for the confinement time, τ, which take a deceptively simple form. An example is the original scaling law of Goldston shown below,

$$\tau_L = .037 c_1 \kappa^{0.5} I P^{-0.5} R^{1.75} a^{-0.37}, \tag{1}$$

The parameters appearing in this expression are the experimental parameters for the device. These laws have a surprisingly large range of validity and have even been able to "predict" some of the advances in tokamak confinement that have been achieved in recent years. Further, they have the ideal, almost seductive feature, from the standpoint of machine design, of using only the gross engineering parameters of the device. To design a tokamak, one simply plugs the engineering parameters into this formula and immediately obtains the confinement time performance. The downside of these empirical laws is that they do not have a physical basis, but are simply unconstrained fits to the existing confinement data. The underlying physical mechanisms causing the heat to leak out of the tokamak are not understood. Thus one cannot say what causes the observed scaling behavior or be confident, that the law will continue to hold, beyond the existing range of data. There is, of course, a great deal of excellent work, both theoretical and experimental, within the program moving toward getting a physical understanding, but at this point the principal questions remain unresolved.

This then is the basis upon which we must now design future machines such as the CIT: an extensive data base with well documented systematic behavior, but uncertainty in extrapolation because the physical mechanisms causing the behavior are unresolved.

Let me now indicate what this means for the CIT by reading the assessment of the likelihood of ignition from our panel report.

MFAC Conclusions Regarding Ignition in the CIT
From the Report of MFAC Panel 22

> Phase I Finding: "Because of the level of performance required and the lack of additional heating power, the overall expectation for ignition in the first phase is very low."
>
> Phase II Finding: "The CIT would have a high probability for ignition in the second phase with only modest enhancements (less than a factor of two) over empirical scaling required. However, without a physics basis for the empirical scalings laws and without known scaling behavior of the enhanced regimes, there cannot be great confidence at the present time in obtaining the performance required for ignition."
>
> General Recommendations: "In spite of the uncertainty in predicting ignition for CIT, the Panel endorses the CIT concept as the proper next step for the fusion program. All the qualitative features known to improve confinement (...) are exploited in the CIT device...In view of the CIT's importance to the nation's fusion program, design, fabrication, and operation of CIT should proceed as a well-coordinated national effort."

The MFAC, to which my panel reported, accepted the technical assessment made for the CIT, but had a somewhat different general recommendation, and expressed strong support for the present CIT experiment (as opposed to the broader CIT "concept" endorsement given by the panel). This position by MFAC reflects the willingness, shared by much of the research community, to accept the risk in the present CIT proposal, because they are interested in the physics that will be learned from the experiment even if it fails to achieve the ignition goal.

There is a further point I would like to make about the CIT to explain the dramatic nature of the risk one takes in building the device within the present level of uncertainty. Ignition requires that the power generated by the fusion alpha particles in the plasma exceed the energy loss rate from the device. But the empirical laws imply that these two competing processes scale with plasma density and temperature in almost exactly the same way. This means that if a device is constructed and one finds that the fusion alpha power fails to beat the energy loss rate, not only will the experiment fail to achieve ignition, but there is very little one can do by varying plasma properties to make it work. Almost arbitrary amounts of additional heating power could be added to the plasma, at great expense, attempting to climb what we call the "heating wall" to no avail. In short, the need for a solid understanding of this behavior is quite acute.

This brings me to describe the two possible approaches which can be taken with the CIT experiment, which I will loosely characterize as phenomenology and physics. In the first strategy, phenomenology, as applied by the current proposal, one aggressively plows forward, arguing urgency of need dominates the possible cost of a mistake. In the other strategy, when there is less perceived urgency and both cost and effectiveness are important considerations, one seeks first to establish a solid technical base. The latter strategy, which I have called the physics strategy, stresses understanding of the dominant phenomena before proceeding with construction of large devices.

The report of my panel summarizes the situation this way in the first paragraphs of its executive summary.

> Serious issues confront magnetic fusion in taking its next step. Very substantial progress in tokamak confinement has been made over the last ten to fifteen years. This has been achieved by a process of experimental exploration of parameters on dozens of different tokamaks worldwide. However, a detailed understanding of the physical processes limiting confinement has remained elusive during that time. Since predictions of confinement performance for future experiments are primarily being made on empirical evidence, such extrapolations cannot be made with the confidence that would flow from an understanding of the confinement physics.
>
> How important is that understanding? Experiments providing data that can advance the goal of attaining fusion power are becoming increasingly expensive. Experimental exploration of parameter space with multiple facilities, each reaching $1B and beyond, is not a viable approach. The data points are simply unaffordable at this price. The physical understanding is needed to assure success in an ignition facility built within budget strictures and to develop the knowledge needed to build an economically competitive fusion reactor.

We have before us an extraordinary opportunity for the US Fusion program. Technical

innovation and progress in tokamak research has brought us to the point where the answers needed to follow the path of physics as opposed to phenomenology could be soon available to us. The high degree of reproducibility and controlability in today's experiments, and the high resolution diagnostic techniques that have been developed now allow measurement in the interior of these very hot plasmas which can answer the key technical questions. In fact this was the first thing I noticed upon returning to the program to conduct this review. There has been a radical improvement in diagnostic capability in the last five years. Further, several even newer ideas have surfaced since we began the Transport Initiative. Three new possibilities for measuring the internal fluctuations so critical to understanding transport were presented to my panel. One has already been implemented on an experiment at the Princeton Plasma Physics Laboratory.

Because of these possibilities, the next 2-4 years could be a time of great scientific accomplishment for fusion. Our panel set specific technical goals for the transport initiative that it believed could be achieved in this period. MFAC concurred in these technical recommendations, in effect agreeing that an aggressive research program could yield the results needed to design the CIT with high confidence in obtaining its ignition goal.

The panel also made a number of specific programatic recommendations in its report, suggesting priorities that might be used to free resources to attack the transport problem. MFAC did not accept many of the panel's inferences of lower priorities for certain activities. In comparing the different recommendations made by the panel and MFAC, please realize that the scope of activities implied by the MFAC recommendations would require funding levels well in excess of the current presidential budget request.

Let me conclude by describing to you briefly the technical nature of the problem to be solved in magnetic fusion in order that you may better understand the complexity of the problem, and the significance to science in general that having solved it will have. Fusion occurs in a state of matter called plasma, often termed the fourth state of matter, at temperatures well above those of the gaseous phase. At these extreme temperatures, comparable to those in the centers of stars, atomic structure is destroyed and the matter is broken down into its more elementary constituents, electrons and nuclei. These charged particle constituents imbue the plasma with strong electric and magnetic forces, making the plasma an extremely complex and dynamic medium. Almost all plasmas exist in a state of continual turbulence.

It is also important to realize that although plasma is a rather exotic state of matter in the terrestrial sphere of human existence, on the cosmic scale, plasma is quite common. In fact, over 90% of the matter in the universe is in the plasma state. Most stars, the magnetospheres of planets, and much of the interstellar medium is plasma. On the cosmic scale, plasma is the dominant form of matter, and by such a measure, plasma physics would be the most important branch of physics. In this context you have heard, as the high energy physicists are fond of saying, that the physics learned from facilities like the SSC will give us some glimpse into the state of things in the beginning of the universe in the first 10^{-x} seconds. What they fail to mention, which is here worth noting, is that the high energy physics picture considers only the elementary particles themselves and that the state of matter in that very early epoch is in fact a plasma (possibly a quark plasma). The behavior of the universe as a whole, not just in the first 10^{-x} seconds, but in the 5 billion years hence, is governed by the laws of matter in a plasma state.

What the U. S. Fusion program is proposing to do in its next steps with the ignition

experiment (CIT) is to quite literally create a burning star and put it in a magnetic bottle in the laboratory. This will require an exceptional understanding of the plasma state of matter — an understanding that has a carry-over to the other more fundamental applications of plasma physics.

Finally, as I hope I made clear, I advocate the path of physics rather than phenomenology in building the CIT, requiring aggressive research now and construction when the needed physics base is in place. The present proposal lacks, in my professional opinion, the physics basis needed to assure success. Proceeding with the requisite design R & D and prototyping while this research is underway, will allow completion of the construction in the FY 96 period and give sufficient time for experimentation to achieve ignition by the turn of the millenium.

The plan implied by my recommendations to you and effectively echoed in the recently submitted report by my Panel, calls for an objective for the U.S. Fusion Program of obtaining a qualitative and quantitative understanding of energy confinement in the tokamak within the 2 - 4 year time frame. This is a very ambitious objective. Its accomplishment would re-affirm the worldwide U.S. leadership in fusion energy science and plasma physics. The construction of an ignition device is the logical and most cost effective next step for the fusion program. This would be the demonstration, for the first time on earth, of a controlled thermonuclear burn. Such a landmark scientific accomplishment and the physics understanding that must accompany it would not only provide insight into the fundamental science issues. It would also open the doorway to a useful potential future energy source.

Senator, I appreciate your attention in letting me give you my views on the fusion program at this hearing.

Attachments to Oral Testimony

Dr. Kim Molvig

Associate Professor of Nuclear Engineering

Massachusetts Institute of Technology

Chairman, MFAC Panel 22

Presented to

The Subcommittee on Energy Research and Development

of the

Committee on Energy and Natural Resources

United States Senate

June 14, 1989

MFAC Conclusions Regarding Ignition in the CIT
From the Report of MFAC Panel 22

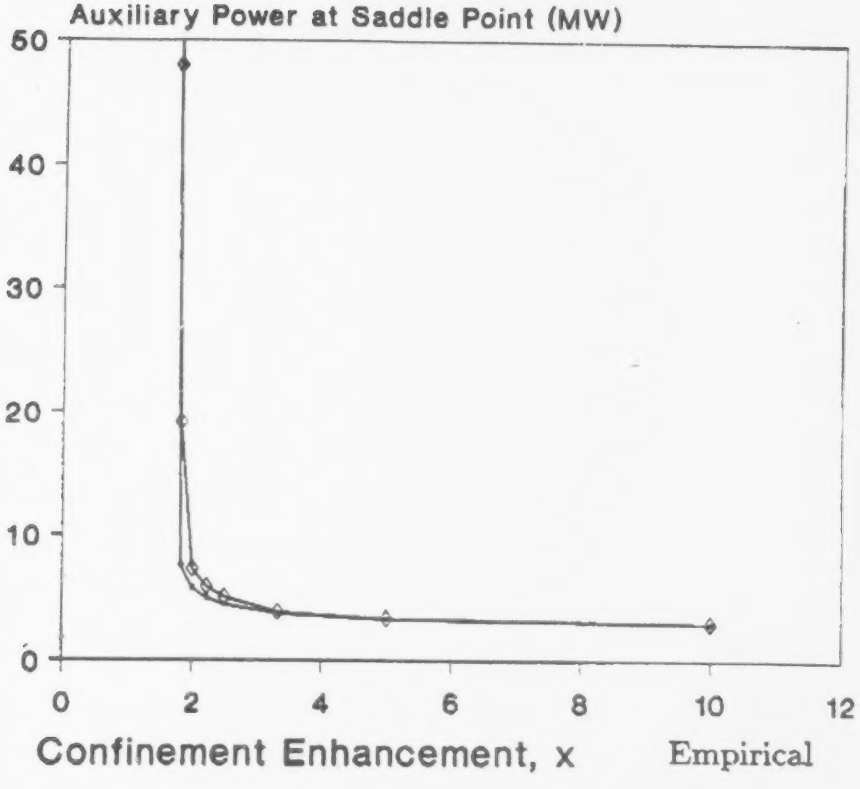
Phase I Finding: "Because of the level of performance required and the lack of additional heating power, the overall expectation for ignition in the first phase is very low."

Phase II Finding: "The CIT would have a high probability for ignition in the second phase with only modest enhancements (less than a factor of two) over empirical scaling required. However, without a physics basis for the empirical scalings laws and without known scaling behavior of the enhanced regimes, there cannot be great confidence at the present time in obtaining the performance required for ignition."

General Recommendations: "In spite of the uncertainty in predicting ignition for CIT, the Panel endorses the CIT *concept* as the proper next step for the fusion program. All the qualitative features known to improve confinement (...) are exploited in the CIT device...In view of the CIT's importance to the nation's fusion program, design, fabrication, and operation of CIT should proceed as a well-coordinated national effort."

(emphasis added)

The Heating Wall
for Ignition



Physics Understanding **is Crucial !**

- MFAC Accepted Technical Assessment

- Differing Programmatic Emphasis —

  Supported Construction of CIT

- Great Opportunity for U. S. Fusion Program

- Innovation and Progress In Tokamaks:

  $\Rightarrow$ Answers needed to follow Path of

  Physics not Phenomenology

Could be Available in 2 - 4 Years (MFAC Concurs)

Presented to the
Senate Committee on Energy And Natural Resources
Subcommittee on Energy Research and Development
June 13, 1989

by

David E. Baldwin
Director, Institute for Fusion Studies
University of Texas, Austin, Texas

Vice-Chairman, Magnetic Fusion Advisory Committee

Mr. Chairman and members of the Committee, it is a privilege to speak to you in support of the magnetic fusion research program. I am David Baldwin, a professor of physics and the director of the Institute for Fusion Studies at The University of Texas at Austin. I am sure you share my sense of fusion's importance in our nation's long-term energy future. The past support for fusion of both the Congress and several Administrations has been most heartening to those of us in the program.

The Institute for Fusion Studies is devoted to long range development of fundamental theoretical understanding of fusion plasma behavior. Its sister experimental organization, the Fusion Research Center containing the TEXT tokamak experiment, is similarly dedicated to examining fundamental processes in laboratory fusion plasmas, particularly the mechanisms causing energy transport. The State of Texas has a long history of support for fusion development, as witnessed by its matching Department of Energy funds for the support of the IFS and for its generous equipment and facilities support for the TEXT.

The stated intermediate objective of the US fusion program is an assessment, soon after the turn of the century, of fusion as a practical energy option. We have an important topic before us today: how best to proceed with the exciting prospect of an ignition demonstration as a key step toward this objective. Accomplishing this long-sought goal of the scientific phase of fusion development

will require marshalling both our material and intellectual resources in a well-coordinated national effort.

I am here as the Vice-Chairman of the Magnetic Fusion Advisory Committee, which advises the Secretary of Energy through the Director of Energy Research on policy relating to the development of magnetic fusion energy. The MFAC is composed of representatives of laboratory and university fusion programs, as well as representatives from industry and utilities.

The MFAC has been a steadfast supporter of the Compact Ignition Tokamak, which is an ignition device exploiting high magnetic field to reduce the machine's size and cost. Recently, MFAC has responded to a charge from Dr. Hunter to re-examine our scientific readiness for the CIT step. Dr. Molvig chaired the Panel 22 that reported to MFAC on this charge. He and the Panel are to be congratulated for a thoroughgoing analysis of this topic.

The Scientific Background for Ignition

The steady progress in plasma confinement physics, particularly over the past twenty years when fiscal support for the program has been strong, removes any real doubt that fusion power is scientifically achievable. The twin tasks remaining before us are actually to demonstrate this scientific feasibility by creating ignition in the laboratory and to develop a reactor that is attractive from criteria of economy, environment and safety.

As a theorist, I am pleased to say that our theoretical understanding of plasmas has developed in parallel with achievements in the laboratory. These advances have been made possible by a combination of good theoretical ideas, sophisticated computer codes to describe these ideas in realistic experimental configurations, and sophisticated diagnostic instruments to test and validate these descriptions. By this process of developing the underlying science of fusion plasmas, concepts only partially understood twenty years ago have now been reduced virtually to

engineering practice. Examples of these include: the large scale, or macroscopic, equilibrium and stability of plasma in magnetic fields (along with the design of these fields); the motion of colliding particles in these complex fields; and the deposition of power injected from outside the plasma. Many aspects of the understanding gained has found application in other fields of technology and science, ranging from defense projects to astrophysics.

Despite these successes, we have not been as successful in understanding the details of the energy transport in plasmas causing energy loss from the plasma at a rate faster than expected. Our experimental progress in this area has been much more empirical, in the sense that we have had to learn in the lab how to confine and heat the plasma in a way to optimize its energy-holding capability -- in effect, by careful observation, we have had to learn from, and work with, the plasma itself.

The empirical knowledge accumulated through this process was used in correctly predicting the performance of today's machines from the data base of their predecessors. A central question is whether today's, or any, empirical base is sufficient to design an ignition experiment, as this must be a large and expensive step. By any standard, any theoretical underpinning to this empirical base would increase our confidence in the result.

There is much in our theoretical picture of transport that correlates well with experiments. The energy transport is almost certainly caused by low level fluctuating electric and magnetic fields forming a turbulent "noise" in the plasma that enhances hot particle diffusion. Actually, the culprit mechanisms have likely been identified in the idealized world of theoretical calculations, but we haven't yet learned how these mechanisms interact with each other and with the particles in real laboratory plasmas. By analogy, although we might recognize individual instruments, we might not be able to recognize them playing in an orchestra.

Because of the gap in our theoretical understanding and the benefits that would accrue for future machines, the fusion program has embarked on a "transport initiative", aimed at marshalling experimental and intellectual resources to make progress in this area.

The genesis for this initiative arose in the MFAC itself in a retreat last summer concerned with planning the program for the early 1990's. It has been greatly helped by Dr. Hunter's redirection of program funds toward this end. Over the last year, all of our major facilities have in varying degrees shifted experimental priority to this issue. The TFTR at Princeton has been able to be particularly responsive, as its fusion-breakeven demonstration using tritium fuel was delayed to provide resources for the transport initiative. A national Transport Task Force has been established (and endorsed by MFAC) to coordinate all these efforts and to assist in the creation of interlaboratory teams of experimenters, theorists and plasma modelers, aimed at specific sub-issues.

The thrust at the physics basis of transport is more than a need for a predictive capability for an ignition facility. It is also an opportunity presented by the maturity of today's theoretical, computational and diagnostic tools. In a sense, the time is now ripe for understanding the area of microscopic phenomena, just as it has been over the past two decades for understanding more macroscopic features.

The Compact Ignition Tokamak (CIT)

The projected uncertainty in the performance of the CIT is generally thought to be at the factor-of-two-level. However, even this affects the facility requirements for ignition. As a consequence, the facility is being designed with flexibility in its construction and operating plan, with the view to keeping the cost as low as possible. The present plan has two phases. The first

phase provides only partial magnet and heating powers and would be sufficient for ignition provided the best of today's experimental results extrapolate. The second phase, if it is needed, would increase the magnet and heating powers as required.

The MFAC Panel 22, charged to examine the question of ignition in CIT, found the expectation of ignition in the first phase to be low, owing to the level of performance required and the marginal nature of the Phase I heating systems. The Panel found a high probability for ignition in Phase II, but warned of the empirical nature of the extrapolation. Success in either phase will require being able to achieve in the CIT (in differing measure in the two phases) one of the so-called "enhanced" modes of confinement operation that have been observed in all of today's major tokamaks. Although not discussed by the Panel, an alternate project approach would provide the full capability at the beginning of operation, with a corresponding increase in confidence of early success.

In recommending proceeding with preparations for the CIT, the MFAC took full cognizance of the Panel findings in a pair of letters to Dr. Hunter. In a letter sent after its meeting March 9, 1989, MFAC said :

> "The Panel 22 Report accurately reflects the uncertainty involved in extrapolating to ignition. It was this uncertainty which gave rise to the two-phase project approach summarized in the report. The first phase provides full coil capability, but only partial powers for the magnets and auxiliary heating, and is intended to sharped the power requirements for the second phase. Additional auxiliary power during Phase 1 would be beneficial toward this end.
>
> In spite of the uncertainty in predicting ignition, the Panel endorses the CIT concept as the proper next step for the program; and, as the general parameter range for the machine is known, it endorses continuation of the necessary design and R&D for the machine. MFAC likewise continues its support for the construction of CIT as expressed on a number of earlier occasions. This will require additional funds in future years.

> The transport initiative will reduce some of the uncertainties regarding ignition. Proceeding at the same time with the construction of the CIT with its basic capability will position us to exploit fully the results of the transport initiative in the experimental phases of CIT."

In a letter sent after its June 7 meeting, MFAC added:

> . . .In the subsequent meeting, Secretary Watkins (has) expressed his support for an ignition experiment within ten years and the need for a physics checkpoint on the transport issue. In response to the sentiments he expressed, we believe that progress on the remaining transport questions will be adequate within the next few years to provide a basis for ignition; that the CIT design, R&D, and prototyping should proceed in parallel with the transport work; and that the check point should release construction funds for the project, rather than initiate them."

As a final comment on the question of "probability of ignition", there is an important point to be made. Any calculation of ignition conditions necessarily entails assumptions about alpha particle behavior and the effects of their heating on the bulk plasma. It is just this physics that the facility is designed to explore, making the CIT a physics experiment. We are, however, able to extrapolate results of experiments using other forms of power input and also to design a margin of safety by exceeding the conditions for ignition as required by our models. The purpose of the transport initiative is to increase confidence in our design criterion and to be as sure as we can that we will be able to raise the plasma to the conditions where intense alpha heating can occur. Then, ignition can follow provided our expectations regarding alpha physics are correct.

What is Needed

The dramatic progress in fusion over the last decade or more has been a result of investments made in the 1970's and early 80's.

Now it is time to look to the needs of the future. Ignition must the fusion centerpiece during the 1990's. However, in targeting an assessment capability in the next 15-20 years, it is important to bear in mind that fusion must also develop the many other aspects of physics, technology, and materials science necessary for a reactor.

In addition to ignition, the physics needs include optimizing the configuration to hold plasma pressure, sustaining the necessary internal current in steady state, and eliminating the phenomanon called disruption, in which the plasma stored energy and current can be lost abruptly. There is good progress to report in all these areas, e.g., the DIII-D beta (a measure of pressure) results reported here.

In technology, we need to develop a range of nuclear technologies special to fusion, in particular, an efficient blanket for breeding the necessary tritium fuel. The proposed International Tokamak Experimental Reactor (ITER) will contribute to this issue in an important way in both its R&D phase and, if constructed, in operation.

In materials science, we need to develop the materials capable of withstanding the neutron and heat loads of a reactor for long periods of service. As emphasized by the MFAC in earlier studies, this is an especially important long-term need that directly impacts the desireablilty of fusion and is not being adequately addressed.

In addition to these directly tokamak oriented activities, the fusion program also supports at much lower levels a number of exploratory efforts, called alternative concepts. These serve both to examine other potential avenues to a reactor and to provide different but complementary views on tokamak physics questions by operating in different parameter regimes. On many occasions, the MFAC has recognized the importance of these activities and their contributions to the health of the program.

It is because of this broad spectum of program needs that the quoted MFAC position recommends incremental funding to complete the CIT project. The recently published NRC study (Pacing the US

Magnetic Program) reported at this hearing also recognizes this need, and it recommends an early funding step of 20% for the ignition objective, along with a 25% step in the later 1990's for ITER participation.

In closing, I return to another point made in the NRC report, that new energy technologies will be required by the middle of the next century and that the capital investment for them will be measured in the trillions of dollars. It will be important to have the best alternatives available when a decision is to be made. Even today, we have come to see energy supply as an essential element of our nation's security. Therefore, I ask for your continued support, especially now that fusion is now nearing its goal. Thank you.

REPORT* ON

CONFINEMENT & MACHINE PHYSICS

MAGNETIC FUSION ADVISORY COMMITTEE

PANEL 22

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David E. Baldwin, University of Texas, Austin
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* This Report was prepared by a Panel established by the Magnetic Fusion Advisory Committee (MFAC). The findings and recommendations presented herein do not necessarily represent the view of MFAC.

June 6, 1989

EXECUTIVE SUMMARY

Serious issues confront magnetic fusion in taking its next step. Very substantial progress in tokamak confinement has been made over the last ten to fifteen years. This has been achieved by a process of experimental exploration of parameters on dozens of different tokamaks worldwide. However, a detailed understanding of the physical processes limiting confinement has remained elusive during that time. Since predictions of confinement performance for future experiments are primarily being made on empirical evidence, such extrapolations cannot be made with the confidence that would flow from an understanding of the confinement physics.

How important is that understanding? Experiments providing data that can advance the goal of attaining fusion power are becoming increasingly expensive. Experimental exploration of parameter space with multiple facilities, each reaching $1B and beyond, is not a viable approach. The data points are simply unaffordable at this price. The physical understanding is needed to assure success in an ignition facility built within budget strictures and to develop the knowledge needed to build an economically competitive fusion reactor.

An increased effort in understanding transport in tokamaks is not only a need driven by the size of future devices, it is also an opportunity presented by features of today's tokamak research: the high degree of reproducibility and controllability in today's experiments, the high-resolution diagnostic techniques that have recently become available and are now under development, and the large body of linear and nonlinear theory of microinstabilities that exists and continues to be developed. As a consequence, top priority in the fusion program has properly been given to identifying and characterizing the mechanisms causing anomalous transport in tokamaks, an effort broadly referred to as the "transport initiative." The physics program has already enthusiastically responded to the transport initiative, starting with the formation of the Transport Task Force (TTF) and through redirections of major portions of its experimental and theoretical research programs. Phase I of the TTF is now completing an extensive assessment of the transport situation, and the real work of tackling the issue will begin this summer in Phase II. Special emphasis in Phase II will be placed on theoretical-experimental and inter-laboratory cooperations. These are appropriate responses to the initiative and augur well for progress in this challenging area.

Although the ability to predict the transport rate is the urgent need for the fusion program, there are other key technical issues to be addressed in order to make tokamak-based fusion reactors more attractive. Examples are efficient current drive, measurement and understanding of central q, high-β (both the first and the second stable region) and fueling. Care should be taken to maintain overall program balance in pursuing the transport initiative.

This report addresses the technical and programmatic issues of relevance to the magnetic fusion program that reflect the need for better understanding of tokamak confinement. The state of our present understanding of tokamak confinement is assessed and recommendations for obtaining an improved physical understanding are made. Due recognition is given to the extent of plasma physics knowledge existing in the fusion program, but not yet developed to the point of impacting machine design. Optimism for near term improvements in physics understanding flows from this theoretical and experimental base. Experimental facilities are reviewed with respect to their capabilities for providing good physics input to the program. Devices ranging from TFTR to university tokamaks, as well as alternate concept facilities are considered. Finally, the report addresses the Compact Ignition Toka-

mak program (CIT). The likelihood of ignition with the first phase of the current design is assessed and found to be quite low. The second phase of the CIT would have a high probability of ignition, provided modest confinement improvements relative to L-mode scaling are achieved. However, there remains an attendant uncertainty resulting from extrapolating on an empirical knowledge base.

Goals for the Transport Initiative

The panel recommends the pursuit of objectives in the transport initiative containing the following general elements. These are very similar to those stated by the Transport Task Force in its Phase I recommendations:

- **Identification of Transport Mechanisms:** Appropriate experimental resources should be brought to bear on establishing the dominant cause of tokamak transport. Is it drift waves, convective cells, magnetic stochasticity, profile adjustment dictated by marginal stability requirements, or some yet-to-be conceived processes? Experimental determinations of free energy sources, scaling properties, and key nondimensional parameters in both standard and enhanced confinement modes are needed to guide theoretical work.

- **Development of a Quantitative Confinement Model:** Analytic and computational models of the dominant confinement processes should be developed, and their parametric dependences validated via simulating the performance of a wide-range of existing discharges in both standard and improved operation, including the criteria for improved performance. Experience shows that success in developing and validating models will require extensive and detailed interactions between theorists and experimentalists. Regression analyses of global properties may not suffice as the interface.

- **Demonstration:** Insofar as possible, performance of proposed new facilities should be tested by operating existing machines in conditions where key nondimensional parameters are similar.

This approach — characterizing and comparing facilities by means of physically-based nondimensional parameters — is similar to the use of the Reynolds' number in fluid turbulence, which provides a very powerful tool even in the absence of a complete understanding of turbulence. For example, one can accurately simulate aerodynamics with scaled models in wind tunnels, since one can reliably scale the observations in the wind tunnel to the real situation, even though the model and reality may differ appreciably. The development of a comparable tool for plasma turbulence could well be the key to successful scaling design of future generation experiments.

Of the many specific recommendations on transport, the panel gives special emphasis to those measurements which can provide qualitatively new data, thereby providing a means for better evaluating theoretical explanations of anomalous transport. For example, new diagnostics which can measure long-wavelength density fluctuations, magnetic turbulence, temperature fluctuations etc., can provide the data that is essential to evaluations of drift mode theories, the importance of trapped ion modes and unfavorable curvature, predictions of strong turbulence saturation, and questions of marginal stability. Because of its likelihood in yielding near term results, the measurement of the density fluctuations in the missing wavenumber range should be given early emphasis.

The panel notes that enhanced confinement is essential to ignition in CIT (Phase II). Therefore, we recommend increased emphasis be given to understanding the transition to these enhanced confinement modes. High priority should be given to detailed measurement of density and potential fluctuations at the plasma edge in H-mode plasmas, and the behavior of these fluctuations during transition.

Appropriate computational facilities will be essential to the development of tokamak transport theory. The Magnetic Fusion Energy Computer Center should evolve into a supercomputer center where charging algorithms favor bonafide supercomputer problems with special access, operating and charging algorithms to facilitate development and running of novel codes designed for simulating the physics of transport relevant microinstabilities. Code development, graphics analysis, etc. should be transferred to local workstations wherever possible.

Major Tokamak Facilities

The U.S. program contains six major tokamak facilities that can now address the key transport questions. In the last six months, the experimental plans of these tokamaks have been increasingly turned to transport issues, so that there is now a heavy emphasis on this issue. Two areas will require continued attention and resources: improved techniques for active plasma modification and perturbation; and improved diagnostics, viz. improved profile measurements on some devices like DIII-D and, more generally, diagnostics instruments measuring internal fluctuations. In addition, the Panel recommends an increased collaboration between university physicists and major tokamak physics teams.

For the present time, the panel sees no unhealthy overlap in the major tokamak facilities. TFTR holds the world record for Q. Its excellent complement of diagnostics allows detailed transport/fluctuation measurements in a reactor-relevant plasma in the near term. PBX-M can explore the physics of the second stability regime, high aspect ratio confinement and the various divertor configurations that will be useful in clarifying H-mode and plasma shaping transport issues. DIII-D has a high-β capability, and is the principal tokamak facility for studying H-mode confinement and L to H mode transitions. Alcator C-MOD, presently under construction, will be the nearest prototype to the proposed CIT device, having nearly the same expected density and magnetic field strength and configuration. TEXT is a medium-sized university tokamak which has already been productively dedicated to transport studies and has unique heavy ion beam diagnostic for measuring density and potential fluctuations with good spatial resolution. MTX takes advantage of a DOD-developed FEL capability to study the interaction of pulsed, very-high-power ECH with a tokamak plasma.

TFTR is the centerpiece of the U. S. fusion program and, in comparison to other U. S. tokamaks, has the largest staff and infrastructure, most extensive diagnostic capability and highest budget. Although a factor-of-two higher than for DIII-D, the operating costs are well below those for JET and JT-60, which form its competition in achieving breakeven conditions. These costs have been subjected to favorable external reviews in the context of its D-T mission by the Management Systems Review Panel (Panofsky, et al.). We recommend re-examining these costs in light of a delayed D-T mission, with special attention to *total* costs incurred by keeping a DT option open. A TFTR with a revised mission aimed only at transport and other non-DT physics issues should have an operating budget below that required for a DT mission.

Within the tokamak program, the PBX-M has a unique capability for exploring the effects of strong shaping and second stability on transport in a high aspect ratio tokamak. It is capable of routine H-mode operation. Continuation of PBX-M operation should be given high priority.

The following table contains a crude ranking of the program's tokamak facilities with respect to their capability for producing physics results on transport that can have an impact on future facilities.

MAJOR TOKAMAK FACILITIES

Facility	Transport Ranking	Annual Oper. Cost[a]	Max. T_i (KeV)	B(T)	R	R/a	Diag-nostics	Divertor	Remarks
TFTR	Exc.	$68M	31	5	2.6	2.6	Exc.	No	World Record Q
DIII-D	Exc.	$35M[b]	6	2	1.7	2.5	Good	Yes	World Record β H-mode
C-MOD[c]	Exc.	$15M[d]	4[e]	9	68	3.2	Good	Yes	H-mode Closest CIT Prototype
PBX-M	Good — Exc.	$10M	5	2	1.7	5.5	Good	Yes	H-mode 2nd Stability High Aspect Ratio
TEXT	Good	$ 6M	2	3	1	3.8	Good	No[f]	Heavy Ion Beam Probe
MTX[g]	Fair	$12M	2[h]	12	.64	3.8	Fair	No	High Power ECH Relies on DOD Funded FEL

[a]Based on 1989 funding including anticipated reprogramming
[b]Includes on-site and off-site support from ORNL (~$1M) and LLNL (~$1.9M).
[c]Under construction.
[d]Includes $4.9M MDF funds.
[e]Expectation with 4MW source ICRF.
[f]Divertor planned for construction.
[g]Previously Alcator C.
[h]Expectation with 2MW FEL.

Alternate Concepts and University Programs

Highlights of the Panel's findings and recommendations on the alternate concepts facilities and University programs are the following.

Although they are at different levels of maturity, all three alternate concepts viewed have produced good physics. Their levels of diagnostic measurements have improved markedly over the last few years, comparable to moderate size tokamaks and, as a result, there have been significant advances in physics understanding. Ranked in order of their potential contributions in understanding tokamaks, the three main concepts being studied are: the Stellarator, the Reversed Field Pinch (RFP), and the Field Reversed Configurations (FRC).

The level of funding of alternate concepts, including university tokamaks and ATF, is approximately 14% of the fusion budget, or 25% of funding for tokamak research. The funding level as a fraction of budget is about right; however, the research activity conducted under this category should be broadened to include more research on fundamental plasma phenomena. This research should go towards innovative research ideas, both in theory and experiment, to advance the frontiers and complement the thinking of the main tokamak line. This fundamental plasma studies program should have some programmatic protection from the more applied portions of the program.

The name "Alternate Concepts" has unfortunately led to support only of experiments which are justified by their reactor potential. We recommend a reassessment of the physics value of present major "Alternate Concept" activities and an effort by DOE to broaden the basic physics base of this activity.

Several small-scale tokamak experiments are operated in university environments, and are typically staffed by small groups consisting of a principal investigator, several graduate students and perhaps a post-doc. A typical funding level is of order of $500 K. Examples of these devices include CCT at UCLA, ENCORE at Cal Tech, HBT at Columbia, VERSATOR II at MIT, RENTOR at RPI and TOKAPOLE at Wisconsin. These experiments play a variety of roles complementary to and supportive of the mainline tokamak effort, but should be considered more broadly as a part of the alternate concepts area. They all have in common the training of graduate students who are the future human resource of the program. The panel recommends maintaining and perhaps strengthening this important program element.

The seminal discovery of the dynamo and other progress warrants the next step in RFP's — the ZT-H device and its associated CPRF facility — which will bring the RFP appreciably closer to reactor conditions. However, the rate of completion of the ZTH is slow because of the need to construct a new facility and the slow pace at which construction funds are made available. The Panel believes that many elements of the ZTH research program will be preempted by the 2-MA RFX at Padova. A new review of the ZTH program should be undertaken to determine to what degree this is true and for reevaluation of the cost effectiveness in this light. The original 4MA machine would provide uniquely new operating regimes. The review should determine whether there is a justification and a clear path to this regime in a manner consistent with information to be obtained from the Padova device and ZTH should the program be continued.

The FRC (although embodying a high-power-density reactor concept) is primarily a basic fusion physics experiment to address such questions as global kinetic effects and transport in purely poloidal magnetic fields. Present experiments have succeeded in producing

large-gyroradius FRC equilibria which have required three-dimensional stabilizing fields. However they have been directed largely toward problems of reactor embodiment and parameter attainment. The next experiments should address the formation and stability of small-gyroradius configurations. Demonstrating and understanding tilt-mode stability in small gyroradius machines is the most important near-term critical issue for the FRC and should be the central program focus. The Panel recommends focussing resources addressing this critical issue. The Panel questions the wisdom of costly facilities at both LANL and STI and recommends a review of the FRC program with attention to these basic stability questions. It further notes that FRC research is well-suited to university experiments. The kinetic equilibrium and stability problems of FRC's constitute challenges to theorists and experimentalists alike, and are well suited to university research.

Summary Epilogue

The plan implied by the recommendations in this report has an objective for the U. S. Fusion Program of obtaining a qualitative and quantitative understanding of energy confinement in the tokamak within the 2-4 year time frame. This is a very ambitious objective. Its accomplishment would re-affirm the worldwide U.S. leadership in fusion energy science and plasma physics. The construction of an ignition device is the logical and most cost effective next step for the fusion program. This would be the demonstration, for the first time on earth, of a controlled thermonuclear burn. Such a landmark scientific accomplishment would open the doorway to a potential future energy source.

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Magnetic Fusion Community Position Paper

May 1989

Introduction

Conservative estimates of world population and energy demand growth show that we can expect serious energy shortfalls within 50 to 75 years. In addition, problems of acid rain, air pollution, and the potential of global warming will necessitate an eventual shift to non-fossil-fuel energy production. During the first half of the 21st century a significant fraction of the world's energy must begin to come from non-fossil fuels.

The development of new energy sources to replace fossil fuel technologies for central station power generation will take decades and cost billions of dollars. Although development funds of this magnitude are large, they are a modest investment relative to the capital investment that will be required for new power plant construction in the first half of the 21st century (an estimated $5 trillion). The federal government must take the initiative in providing substantial and timely support for the development of new energy sources to replace present fossil fuel technologies.

Among the possible long-term energy sources, fusion is a major candidate. At the 1985 Geneva Summit Meeting, President Reagan and General Secretary Gorbachev "emphasized the potential of the work aimed at utilizing controlled thermonuclear fusion for peaceful purposes and, in this connection, advocated the widest practical development of international cooperation in obtaining this source of energy, which is essentially inexhaustible, for the benefit of mankind." This policy, reconfirmed at subsequent summit meetings and through lower-level meetings among nations, many of which already have strong fusion energy programs in place, has resulted in the formal establishment of a joint, multinational effort to design the world's first fusion engineering test reactor. Such a facility, called the International Thermonuclear Experimental Reactor (ITER), could be constructed during the 1990's.

If a fusion engineering test reactor can be completed around the year 2000, as a complement to strengthened national fusion science and technology programs, a fusion power demonstration plant could be operational in the first quarter of the next century.

The Science of Fusion

Fusion research has resulted in the development of a whole new branch of science: high temperature plasma physics. Unlike many basic science programs, high temperature plasma physics has an additional major practical goal: achieving fusion power.

Fusion requires that a hydrogen gas (plasma) of a certain density be raised to a high temperature. The plasma must be insulated from its surroundings (by a magnetic field) to keep it from losing its heat too rapidly. By far the most mature fusion concept is the tokamak, but there are other closely-related configurations. These configurations may be distinguished by their characteristic plasma current densities, ranging from the high-current reversed field pinch, to the moderate-current tokamak, to the null-current stellarator. By studying a range of magnetic configurations, we hope to improve upon our basic understanding and evolve increasingly more attractive fusion concepts.

The magnetic fusion effort has been remarkably successful, as evidenced by:

- The systematic progress in our ability to produce the plasma conditions required for fusion power, including fusion-grade plasmas at temperatures of over 300 milion degrees.
- Our ability to explain and to predict many aspects of the complex behavior of magnetically-confined plasmas.

Progress toward fusion can be illustrated (see figure) by comparing the equivalent fusion power produced to the rate of heat loss from the plasma. Tokamak experiments have increased this ratio by a factor of over a million during the last 20 years, by raising the plasma temperatures and by improving the quality of heat insulation. A factor of only about 10 remains to be achieved in order to reach the "ignition" point, where the plasma keeps itself hot by means of its own fusion reactions, while generating a large power output as well.

This goal of ignition is now within our grasp and could be achieved during the 1990's. A facility has

been proposed, called the Compact Ignition Tokamak (CIT) to produce such a plasma.

Early progress in the fusion program tended to be experimentally driven; however, as the program achieves its performance goals, the focus shifts to issues of efficiency, cost, and optimization. In the current phase, the fusion program will benefit from a closer coupling of theoretical understanding and experimental demonstration.

Many points of agreement between theory and experiment are documented in numerous plasma physics textbooks now used in our universities and in countless research papers published in scientific journals. A primary example is the magnetohydrodynamic (MHD) description of plasma equilibrium and stability. We have now reached the point where we can predict with great confidence the equilibrium and macroscopic stability properties of present and future devices.

Progress in understanding the detailed physical mechanisms responsible for plasma heat loss has not kept pace. Despite substantial efforts, this aspect of fusion research is today the most empirical part of the program. Energy and particle loss by both ions and electrons usually involves the cumulative effect of small-scale turbulence, which is still not well understood. However, important progress has been made in the last few years. The progress has been facilitated by many developments: powerful diagnostic instruments for measuring plasma behavior, important new concepts and tools in plasma theory, and the computer programs necessary to analyze realistic laboratory situations. As plasma properties are measured with increasing precision, in more realistic fusion plasma experiments, a qualitative relationship to the predictions of transport theory has begun to emerge.

The U.S. fusion community recently established a national initiative (Transport Task Force) to focus attention and effort on the energy transport issue and to seek means for improvement of plasma confinement through better characterization and understanding of the underlying processes. We expect that important advances in our ability to measure and characterize transport phenomena can be made in the next few years. This will allow us to better assess fusion's potential and possibly identify means to reduce plasma energy loss.

One important area of fusion science in which existing experimental facilities are inadequate is the investigation of the physics of a plasma sustained by internally generated fusion energy -- the physics of ignition by alpha particle heating. The purpose of the proposed CIT is to ensure that we develop experience and understanding in this important area. The design of this device will be improved as results come in from the Transport Task Force and from ongoing experiments.

The development of the optimum fusion configuration requires at least moderate sized tests of the non-tokamak elements of the toroidal class. In the near-term they contribute to the understanding of overall toroidal physics, and in the long-term they may contribute to the development of a superior reactor concept.

The Technology of Fusion

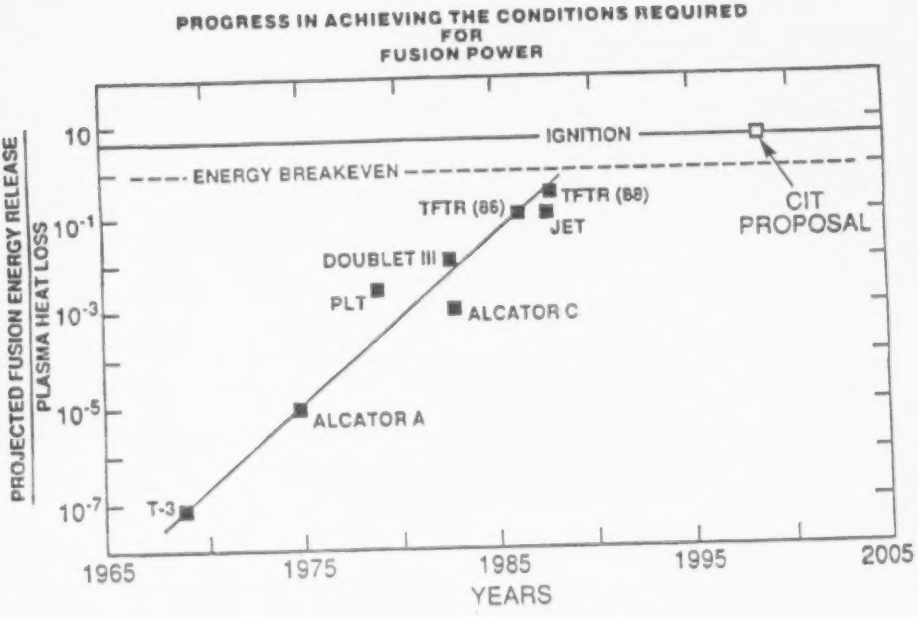
The ability to develop and exploit new technologies has paced scientific advances in fusion research. Notable examples include vacuum conditioning techniques, normal conducting and superconducting magnets, plasma diagnostics and control systems, advanced computers, multi-megawatt neutral beam and radio-frequency heating systems, and high speed hydrogen pellets for plasma fueling. Most of these developments were the fruits of separate technology R&D efforts.

Past technology work has focused on producing and sustaining plasmas, but with a moderate and successful effort on the development of materials and nuclear technology. The development of fusion nuclear technology and materials will substantially influence the economic, safety, and environmental potential of fusion. Since the necessary technology will take many years to develop, a more aggressive program should begin immediately. A key facility needed for the future is a fusion neutron source facility for irradiating and testing new materials.

Near-Term Development Plan

The objective of the U.S. magnetic fusion program is to establish the scientific and technological data base for an assessment of the economic potential and environmental aspects of fusion energy. Attaining this fundamental goal of the fusion program requires meeting a set of important near-term objectives:

- Achievement of an improved theoretical basis for energy and particle transport in toroidal systems, consistent with the experimental data base, to establish a better predictive capability for future machine design.
- Demonstration of improved energy confinement, with better experimental characterization, in existing toroidal systems; this includes continued tests in non-tokamak and advanced tokamak devices such as DIII-D, ATF, and CPRF.
- Advancement of the plasma performance of the Tokamak Fusion Test Reactor (TFTR) toward equivalent breakeven conditions, and maintenance of the option of utilizing deuterium-tritium in a breakeven plasma.
- Construction of an ignition tokamak to pursue ignition physics based on alpha particle heating.



- Completion of engineering design for the tokamak-based International Thermonuclear Experimental Reactor (ITER), supported by validating R&D.

- Initiation of an aggressive fusion nuclear technology and materials program, in anticipation of the requirements for a demonstration power plant.

- Performance of appropriate systems studies, to help guide the research and technology programs.

In pursuit of these objectives, it will be important to maintain a base program that encourages innovation, supports strong programs in universities, and promotes the participation of industry. The long-term health of the fusion program depends on a continuing supply of new talent and ideas and the development of an industrial infrastructure. Fusion research needs to attract first-rate students and to ensure their high-quality training. A balanced program, involving on-site experiments at national laboratories, universities and industries and the teaming of universities, industries and national laboratories, is desirable.

Accomplishing these objectives will require enhanced funding over the next decade, but promises to result in a superior fusion concept with lower technical risks and in lower development cost in the long run.

The Challenge

To maintain its place within the emerging international partnership in fusion R&D, the U.S. faces two critical challenges:

The first challenge, especially over the next five years or so, is to make the best use of existing facilities and to continue to develop critical scientific and engineering data. A strong U.S. contribution to the ITER design is necessary to meet cost and performance goals and to retain a significant U.S. role. This effort should capitalize on those attributes of the U.S. program that have contributed to the success of fusion research to date.

The second challenge is to maintain a productive U.S. program in the longer term. All governments participating in the ITER process have recognized the importance of maintaining a strong domestic program in order to remain viable members of the

international team. The history of the fusion program shows that this requires aggressive development of technologies and higher-performance plasma confinement facilities to produce plasmas ever closer to the reactor regime. The U.S. fusion community has reached a consensus that the next major facility needed in the U.S. program is an ignition tokamak. The international fusion community has recognized that an ignition tokamak can provide unique experience in operation and control well before the start of operation with ignition D-T plasmas in ITER.

As budgets have declined in the 1980's, the fusion program has been reaping the rewards from the investment in facilities and ideas that occurred in the 1970's. We are left with many productive facilities operating at reduced levels owing to inadequate operating budgets. This results in an inefficient use of the taxpayer's previous investment and it significantly retards the rate at which progress in understanding and demonstration occurs.

Further progress toward achieving fusion will require new facilities utilizing the advances in physics and technology that have already been acheived. The Compact Ignition Tokamak (CIT) has been proposed as a moderate-cost next step facility. Its construction will require some growth in the total fusion budget. There is a broad consensus that funds for CIT construction should not be sought by cutting further into the balance of the program, since such cuts would damage the prospects for progress in other key fusion development areas. These areas include advanced plasma confinement concepts, advanced technologies and materials, and innovative basic research. These efforts need to be maintained as the essential foundation for the advances in science and technology that will eventually lead to the timely commercialization of fusion power.

Participants

The following members of the U.S. magnetic fusion community participated in the preparation of this paper:

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The CHAIRMAN. Our next panel is Dr. Dan Dreyfus, whose name we have heard somewhere, who is Senior Vice President of Strategic Planning and Analysis of the Gas Research Institute, and Dr. Peter L. Auer, Professor of Physics at Cornell, on behalf of the National Research Council's Magnetic Fusion Advisory Committee.

Gentlemen, welcome to the committee. Dan.

STATEMENT OF DR. DANIEL A. DREYFUS, VICE PRESIDENT, STRATEGIC PLANNING AND ANALYSIS, GAS RESEARCH INSTITUTE

Dr. DREYFUS. Mr. Chairman, I appreciate your invitation to present a statement to the subcommittee concerning the magnetic fusion program. I am Dan Dreyfus, and I am with the Gas Research Institute. However, today, along with Dr. Pete Auer of Cornell, I am here in the capacity of representing the Committee on Magnetic Fusion in Energy Policy, which was appointed by the National Academy of Sciences at the request of the Department of Energy to conduct a study of the priority and pace of magnetic fusion R&D in the context of long-term national energy policy.

The committee conducted its studies over a period of about 18 months, and the report has just recently been issued. The report is available to you, and I have a list of committee members attached to my statement.

I think that our committee's role was very different from some of the discussion you have had thus far in that we did not delve into the technical merits of a particular experiment. Our charge was to review the magnetic fusion program and its objectives in the context of its probable contribution to a national energy program.

I believe that the results of the committee's study, while they will not shed much light on whether CIT will work or not, are significant to the grander question of what you do with this program as you consider the initiatives that came up, of course, much more recently than the conclusion of our work.

One very important consideration is that the U.S. magnetic fusion program has been defined to be an energy program rather than simply a scientific program to investigate the phenomena of fusion. The existing program has been planned and justified as a contribution to a national, long-term energy policy.

The committee believed—and I still believe—that any evaluation of that program must examine the goals of the program in terms of the likelihood that fusion energy will play an appropriate role in a long-term U.S. and global energy supply mix. If, on the other hand, fusion turns out to be infeasible, commercially or technically, the question becomes whether this program will help current and future policymakers and industrial decisionmakers make the public and private decisions necessary to serve the national energy policy.

That policy is currently, unless it has been changed in the last few days, is stated to be to ensure an affordable, safe, environmentally acceptable and stable supply of energy to support a growing economy and improve standards of living. Successful commercialization of fusion energy is not expected until after 2050, but important societal decisions involving major political and financial commitments to energy supply will probably be confronted earlier.

Vital decisions may have to be made much sooner than the 21st century, and they could be influenced by the preliminary results of the fusion program.

We did take a look at the future consumption and supply mix, and obviously, any contemplation of that future beyond half a century involves many uncertainties. But I think there are some truisms that can be determined from the trends today and the probabilities today.

The outlook for energy supply and demand until the year 2020, which is the most horizon for most contemporary projections, includes some very probable trends and some significant uncertainties. The trends are: continued efficiency improvements in the uses of energy, but at rates much less dramatic than our most recent experience; increasing significance of the developing economies in the global energy demand mix, and they will have higher intensities of energy use because of urbanization; increasing per capita levels of services; and shifts from unaccounted for, noncommercial energy to the commercial fuels.

The result is an increase in the average global energy intensity. Even though both the developing economies and the developed economies will improve their efficiency, the change in mix means that the average gets less efficient. And a continuing increase in the share of energy services provided by electricity. This is a truism, and likely to continue, and is important to contemplating the fusion option.

[The prepared statement of Dr. Dreyfus follows:]

Statement of Dr. Daniel A. Dreyfus

Vice President

Strategic Planning and Analysis

Gas Research Institute

before

The Subcommittee on Energy Research and Development

of the

The Committee on Energy and Natural Resources

United States Senate

for

a Hearing on

The Department of Energy's

Magnetic Fusion Energy Research,

Development and Demonstration Program

June 14, 1989

Statement of Dr. Daniel A. Dreyfus
Vice President
Strategic Planning and Analysis
Gas Research Institute

before

The Subcommittee on Energy Research and Development
of the
The Committee on Energy and Natural Resources
United States Senate

for

a Hearing on
The Department of Energy's
Magnetic Fusion Energy Research,
Development and Demonstration Program

June 14, 1989

Introduction

Mr. Chairman, I appreciate your invitation to present a statement to the Subcommittee concerning the Department of Energy's Magnetic Fusion R&D Program.

I am Dan Dreyfus, Vice President for Strategic Planning and Analysis of the Gas Research Institute. Along with Dr. Peter L. Auer of Cornell University, I am appearing today as a member of the Committee on Magnetic Fusion in Energy Policy, which was appointed by the National Research Council, an arm of the National Academies of Sciences and Engineering and the Institute of Medicine.

The committee was established in response to a request by the Department of Energy for the National Research Council to:

> "Conduct a study of the priority and pace of magnetic fusion R&D in the context of long-term national energy policy and the factors that should enter into that policy formulation."

The committee conducted its studies over a period of about 18 months. The report, entitled "Pacing the U.S. Magnetic Fusion Program," was released on June 1 and copies have been made available to you. The committee was chaired by Dr. Irvin White, President of the New York State Energy Research and Development Authority. A list of the committee members is attached to my statement and biographies are included in the report.

Five specific tasks were specified for the study:

1. Review long-term energy policy implications for current Magnetic Fusion Energy R&D;

2. Identify factors for reducing long-term electricity supply risks;

3. Propose R&D criteria to achieve long-term energy policy goals;

4. Apply criteria and relate them to the priority and pace of Magnetic Fusion Energy R&D; and

5. Present findings, conclusions, and recommendations.

In my presentation, I will summarize the approach taken by the committee and highlight some general conclusions regarding the energy policy setting and the need for a magnetic fusion energy option. Dr. Auer will then discuss the program alternatives that the committee considered, the implications of each, and the committee's recommendations.

Results of the Study

In its approach to the study, the committee reviewed the current magnetic fusion energy program and its objectives, the probable energy supply and demand outlook, both in the U.S. and globally, along with the uncertainties inherent in that outlook, and some selected energy supply alternatives for the long-term future. In the context of these considerations, we analyzed several fusion program alternatives and made our recommendations.

Within the constraints of the tasks assigned to the committee and the available resources, we did not evaluate the scientific and technical attributes of the program, only its pragmatic elements. Neither did we endorse an independent forecast of the energy future or make a comprehensive evaluation of technical alternatives to fusion energy.

The Role of Fusion in the Energy Outlook

The U.S. Magnetic Fusion Program has been defined to be an energy program, rather than simply a scientific program to investigate the phenomena of fusion. The existing program has been planned and justified as a contribution to a national, long-term energy policy. An evaluation of the program, therefore, must examine the goals for the program in terms of the likelihood that fusion energy will play an appropriate role in the long-term U.S. and global energy supply mix; or, if fusion is not used, the likelihood that the results of the program will enhance the ability of current and future policymakers and industrial decisionmakers to "ensure an affordable, safe, environmentally acceptable, and stable supply of energy to support a growing economy and improved standards of living."

Successful commercialization of magnetic fusion energy is not expected until after the year 2050. Important societal decisions involving major commitments to energy supply alternatives may be confronted earlier, however. Current discussions about the management of global warming trends, for example, highlight the policy significance of the world's commitments to long-term energy options. Vital decisions which may be made much sooner than the mid-21st century could be influenced by the preliminary results of the fusion program.

Historical Energy Use

The future of energy consumption and the probable supply mix, particularly on a time horizon exceeding half a century, involve many uncertainties. On the other hand, there are also some constants and some inertia in the system which provide a basis for contemplating even the long-range future. Fundamental energy services, heat, light, motor drive, communications and control are predictable needs of future generations. The requirements for these services are linked, however loosely, to demographic trends and economic activity. There are technical and economic considerations that dictate the choices of energy sources for particular services and that change slowly, if at all. The existing infrastructure that produces, transports and converts energy is immense. It can be replaced only gradually and at great expense. Its stability is further reinforced by a global establishment of business organizations, trade relationships, and vested interests.

Technology is the link between energy sources and energy services. It dictates the technical and economic constraints on the supply mix and it controls the overall efficiency of the system. Technology evolves, but it evolves only slowly. It has taken about 45 years for nuclear fission energy to emerge from a proven concept to 5 percent of global energy supply.

Table 1 demonstrates that over a 12 year period, beginning at the height of the energy crisis, and encompassing an unprecedented period of energy price shocks and direct policy intervention into the energy sector, the global mix of energy supplies has changed, but only marginally.

Table 1
World Primary Energy Consumption

	1975		1987	
	Quads	%	Quads	%
Oil	109	46	118	38
Gas	43	18	62	20
Coal	68	29	95	30
Nuclear	3	1	16	5
Hydro & Renewables	14	6	21	7
Total	237	100	312	100

The principal recent trends have been:

- o greatly decreased energy intensity which diminished the earlier rates of energy demand growth in industrialized economies, though this trend was not experienced in developing countries; and

- o a reduction in the share of the energy supply provided by oil, which is principally attributed to the replacement of fuel oil by nuclear and coal in the generation of electric power.

Both of these trends have shown evidence of moderating or ending over the past two or three years.

Another significant trend which is not evident on the table has been an increasing proportion of energy services provided by electricity at the point of use. The generation of electricity now accounts for more than 34 percent of global primary energy demand and the proportion is very likely to increase.

Many of the most rapidly growing energy service requirements such as lighting, refrigeration, communications, and small motor drive are inherently electric. Furthermore, these are among the most vital services needed to accommodate urbanization and improved standards of living in developing economies and to offer greater consumer options in the centrally planned economies. These two groups of countries are likely to establish the pace and character of future global energy demand growth.

Evolution of the Energy Situation: The 2020 Decision Point

The outlook for energy supply and demand until the year 2020, the horizon for most contemporary projections, includes some probable trends and some significant uncertainties.

Among the trends are:

- o continued efficiency improvements in the uses of energy but at rates less dramatic than recent experience;

- o increasing significance of the developing economies in the global energy demand mix, having higher intensities of energy use because of urbanization, increasing per capita levels of services, and shifts away from unaccounted for non-commercial (or traditional) fuel sources;

- o a resulting increase in the average energy intensity of global economic activity;

- o a continued increase in the share of energy services provided by electricity;

- o large increases in coal consumption to serve electric power generation; and

- o no global shortages of major energy resources at rational price levels, but a return to Middle Eastern dominance over the world oil trade.

Among the principal uncertainties are:

- o unforeseen constraints on one or more major energy sources, for example:
  - abandonment of nuclear-electric programs because of safety or weapon proliferation concerns,
  - constraints on coal use for environmental considerations,
  - economic or political disruption of the world oil trade, or
  - failure of expected efficiency improvements; and
- o major variations from the expected economic outlook, for example:
  - a protracted global recession,
  - a repetition of the economic growth of the 1950s and 1960s,
  - stagnation of Third World economies, or
  - major entrance of the centrally planned economies into global trade as suppliers and consumers of energy or energy-intensive goods.

If energy demand growth should prove to be slower than expected, the result would be only to postpone the projected levels of energy requirements along the time scale. The eventual pressures on the conventional energy supply system would be postponed but not eliminated.

Should the uncertainties work in the other direction, to increase demand or constrain supply, serious policy issues will emerge sooner. The most intractable policy issues would arise from an unanticipated constraint on energy supply. An example of one such constraint would be a decision to moderate global climate modification trends through limitations on fossil fuel consumption.

There is a strong possibility, therefore, that in the early half of the next century, policymakers will confront a shortfall in the availability of conventional energy sources, particularly fuels for conversion to electricity. A public decision may be required, in the U.S. or globally, to make a major social and economic commitment to the commercialization of a new energy supply source.

The Long-Term Energy Situation

By 2050, when magnetic fusion may become a viable choice for a commercial energy source, the stress on conventional energy sources will be significant. This is so even in a scenario, such as shown on Table 2, which assumes no major dislocations of supplies or non-technical constraints on the availability of conventional energy sources.

Table 2
Long-Term Primary Energy Requirements
(Quads)

	1986	2050
OECD	150	301
Developing Countries	46	229
Centrally Planned	105	250
World Total	301	780

Energy requirements will be approaching equivalence among the developing economies, the industrialized West, and the centrally planned economies. Energy consumed to generate electricity will dominate the demand situation. If non-utility generation is considered, it will amount to well over half of all primary energy required.

About half of the currently known world oil reserves will be consumed in the interim. Petroleum will be a costly resource assigned primarily to feedstock and mobile equipment uses where its advantages are unique.

Global natural gas production will be concentrated in the Middle East and the U.S.S.R., adding to the strategic and economic concentration of the oil trade.

Coal and nuclear energy will be the mainstay of electric generation, if they remain unconstrained.

Certainly, new technologies not yet apparent could be proven and could penetrate energy markets by the mid-21st century. Energy conservation technologies could reduce demand substantially. But it is equally probable that technologies for new energy uses could have an opposite impact, increasing energy requirements as air-conditioning did in the 1950s and 1960s and as communications applications are now doing. Emerging technologies such as superconductive materials might enhance the management and efficiency of electric power generation, transmission, and use, and diminish the rate of growth of primary energy demand.

As in the earlier decision point, however, the probability seems larger that unforeseen circumstances will exacerbate the pressures on conventional energy sources rather than relieve them. Again, constraints or dislocations affecting the oil, coal, or nuclear contributions to the supply mix loom large.

The need for, and the commercial viability of, magnetic fusion as a major contributor to U.S. and global energy will be determined in this context.

It seems very probable that a new energy source will be urgently sought. Although current concepts of commercial magnetic fusion appear costly in comparison with the probable evolutionary technologies that will then be available to use coal and fission nuclear energy, the possibility that these sources will not be universally available dominates the outlook.

The committee concluded that a prudent, long-term energy strategy for the United States requires that alternative electric energy supply technologies be researched, developed and demonstrated. A diversified array of alternatives is needed as insurance against the vulnerability of existing energy supply sources. Magnetic fusion merits continuing support as a potentially important option in such an energy insurance strategy.

Thank you, Mr. Chairman. That completes my prepared remarks. Dr. Auer will conclude our presentation by discussing the committee's conclusions concerning the alternative magnetic fusion program levels. We ask that the executive summary of our report and such other portions of the report as the committee may deem appropriate be included in the hearing record at the conclusion of our statements and we will be pleased, of course, to respond to questions.

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Pacing the U.S. Magnetic Fusion Program

A Report Prepared by the
Committee on Magnetic Fusion
in Energy Policy
Energy Engineering Board
Commission on Engineering and Technical Systems
National Research Council

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EXECUTIVE SUMMARY

This study addresses the priority and pace of the nation's magnetic fusion research and development program in the context of long-term national energy policy. In particular, the committee interpreted its task as follows: To review the implications of long-term national energy policy for current research and development in magnetic fusion; to identify factors that should enter the further development of such policy to reduce risks associated with the future electricity supply system; to propose criteria applicable to research and develop in electric generation in reaching long-term energy policy goals; to apply these criteria to magnetic fusion and alternative electric generation technologies in order to develop recommendations on the priority and pace of the magnetic fusion program; and to present its results in a final report.

The most important goals of the U.S. Department of Energy's current Magnetic Fusion Energy Program Plan are to demonstrate the scientific and engineering feasibility of fusion. Demonstrating engineering feasibility will require the design, construction, and operation of an engineering test reactor, which the plan envisions financing through a combination of domestic and international funding. The committee believes that current domestic program funding levels are inadequate to meet even the near-term objectives of the plan.

The committee considered four programmatic alternatives:

- Asserting U.S. scientific and engineering leadership by pursuing a broad-based, relatively independent U.S. program that would minimize U.S. participation in international collaborative efforts. Developing a solely domestic engineering test reactor would require funding levels substantially greater than current levels;

- Maintaining present program objectives, which the committee believes will require increases in funding above current levels;

- o Maintaining current program funding levels, which will require either reducing present program breadth or deferring both the construction of a U.S. facility for conducting ignition experiments and U.S. participation in the construction of an international engineering test reactor; or

- o Reducing current program funding levels, which will require narrowing the scope of the present domestic program and withdrawing from international collaborative efforts.

THE LONG-TERM ENERGY FUTURE AND ENERGY SUPPLY CONTEXT

Fusion power plants would generate baseload electricity. Consequently, they will most likely have to compete with coal-fired and nuclear fission plants employing advanced technology that can be expected to be commercially available by 2050. Other baseload technologies, such as solar photovoltaic systems including storage, might also be commercially available by that time. In principle, coal-fired, fission, and other nonfusion power-generation alternatives could supply global electricity needs in that time frame. For example, the committee finds (Chapter 3) that by year 2010 the use of coal, the most abundant fossil fuel, is projected to increase by 50 percent in the United States, and to double in market economies. However, the future of each of these technologies is uncertain. For example, coal combustion is known to contribute significantly to atmospheric carbon dioxide accumulations, further generation of electricity using nuclear fission must overcome the loss of confidence that has developed over the last two decades, and new fission technologies have not yet been reduced to commercial practice, though there is little doubt of their future commercial viability (Chapters 3 and 4).

The committee finds (Chapter 3) that today's principal end uses of energy--transportation; heating, cooling, and other residential and commercial applications; lighting; heat and power for manufacturing applications; and industrial feedstocks--will continue in the future. The proportion of total energy requirements served by electricity will continue to grow, and electricity will account for an increasingly larger share of primary energy consumption. The mix of U.S. energy uses dictates the mix of U.S. energy supplies, and changes will be slow because of a vast existing infrastructure. In the United States and other industrial nations, the current excess capacity for generating electricity is likely to disappear during the 1990s. By reducing consumption, conservation and energy efficiency measures can extend conventional energy supplies and lengthen the horizon for developing and commercializing alternate sources of energy. However, the committee concludes that these measures are unlikely to eliminate the need for new energy options during the next century.

The committee further finds and concludes (Chapter 4) that the future mix of primary energy supplies will depend on both energy and nonenergy considerations, including 1) economics, 2) environmental health and safety aspects of energy supplies, and 3) unforeseen political developments. The use of widely available fuels may be restricted. For example, as discussed in Chapter 3, coal is now the focal point of global controversies over acid precipitation and global warming. A critical priority for decision makers is to understand what responses to make to acid precipitation and to the possibility of global warming.

The committee concludes (Chapter 4) that a prudent long-term energy strategy for the United States requires that alternative electric energy supply technologies be researched, developed and demonstrated. A diversified array of alternatives is needed as insurance against the vulnerability of existing alternatives, such as coal and nuclear fission.

Thus, the committee concludes that the United States should adopt an energy-insurance strategy, developing an array of diverse technologies as hedges against serious shortfalls in coal-derived and nuclear fission-derived power. As a part of this study, the committee surveyed broadly the prospects for a number of technologies, but within study constraints was not able to assess their relative merits as a basis for allocating federal R&D funds. Thus the committee recognizes a need--not fulfilled by this study--to conduct a comprehensive comparative evaluation of demand management and electrical supply alternatives, leading to the formulation and implemention of a balanced national energy R&D program. Pending such an evaluation, the committee believes that magnetic fusion merits continuing support as a potentially important technological alternative to resolve energy-supply uncertainties such as those noted above.

THE MAGNETIC FUSION PROGRAM IN CONTEXT

The goals of the U.S. magnetic fusion program appear reasonable to the committee. The committee finds (Chapter 2) that as an energy program, the primary objective of the Department of Energy's Magnetic Fusion Energy program is to establish the scientific and technical base for magnetic fusion by about 2005-2010 to support subsequent commercial reactor design and demonstration. This timing is coordinated with cooperative U.S. efforts toward an international demonstration of engineering feasibility.

The committee further finds (Chapter 2) that the United States is no longer the dominant leader in international research on magnetic fusion. The United States lost its leadership position in magnetic fusion energy to the European Community largely because of a 50-percent decrease in funding (measured in constant dollars) since 1977. As a consequence, the U.S. program has also narrowed. Although "energy breakeven" in the next few years is a widely shared expectation, fusion ignition and burn remain to be demonstrated.

The next major frontier in magnetic fusion development is the achievement of a self-sustaining fusion plasma; namely, fusion, ignition, and burn. The next critical-path experiment proposed by the Department of Energy is the Compact Ignition Tokamak. If successful, the Compact Ignition Tokamak will have demonstrated fusion's scientific feasibility. After Compact Ignition Tokamak, the Department of Energy strategy is to participate in an international program to establish the engineering feasibility of fusion.

The committee concludes (Chapter 5) that if stated U.S. fusion program objectives are to be achieved, funding levels higher than current levels will be required. Annual funding would need to increase by about 20 percent in the near term, then by about another 25 percent during the mid- to late-1990s. Such levels would be required to enable the United States to proceed now with a fusion ignition experiment and to participate later as an equal partner in the construction of an international experimental facility intended to demonstrate engineering feasibility. These steps could also provide the United States with an opportunity to develop a U.S. industrial cadre skilled in fusion R&D, thus preparing industry for future participation in fusion development.

The committee considered (Chapter 5) but rejected three fusion program alternatives. One such alternative would be to pursue a broad-based, relatively independent U.S. program aimed at asserting U.S. scientific and engineering leadership. That program would cost appreciably more than the current program, requiring annual budgets about twice current levels to embark on the construction phase of a domestic engineering test reactor in the mid-1990s. The committee also rejected the alternative of limiting funding to current levels. That alternative would require narrowing the breadth of present program activity and removing one or more major planned program elements, thus decreasing the likelihood that magnetic fusion energy will be a product of U.S. technological development. Finally, the committee considered and rejected the alternative of reducing funding, which would eliminate further U.S. collaboration as an equal partner in the international fusion effort.

The United States, the European Community, Japan, and the Soviet Union have separate programs to demonstrate the scientific feasibility of magnetic fusion. In person-years per year, the current U.S. and Japanese fusion programs are comparable in effort, while the European Community's program has significantly greater activity. The size of the Soviet Union's fusion program is difficult to estimate, but appears to be comparable to that of the United States. Largely because of the high cost of fusion research and development, international collaboration and cooperation are increasing; the committee endorses this trend and believes that the U.S. fusion program should be based on a strong international collaborative effort (Chapter 5).

Currently, under the existing international collaborative effort, there is considerable industrial participation in the Japanese fusion program, where industry plays a major role in designing and building

fusion systems and subsystems. This attribute is largely absent from the U.S. program. If this lack of parallelism continues, U.S. companies will be at a competitive disadvantage as the fusion research and development program is increasingly internationalized and as fusion and its related technologies move toward commercialization.

COMMITTEE RECOMMENDATIONS

Based upon the considerations and study conclusions summarized on the preceding pages and in the body of this report, the committee has arrived at the following recommendations:

- The committee recommends that the federal government develop a broad, balanced array of technological alternatives as an insurance strategy for meeting U.S. and global long-term energy needs. Decisions about the government's role, which alternatives to support, and funding levels should be based on the results of a comprehensive assessment. That assessment should include the full range of energy, environmental, public health and safety, security, economic, institutional, and other attributes of each technological alternative, including magnetic fusion (see Chapter 4). In the interim, the committee offers the three recommendations below as to the priority, pace, and direction of the U.S. magnetic fusion program.

- The committee recommends that the United States enter into partnership arrangements for the next major steps in an international fusion program, as the most cost-beneficial U.S. approach to fusion over the next decade (see Chapter 5).

- The committee recommends an increase over current fusion program funding of about 20 percent, held steady for the next five-years, followed in the mid- to late-1990s by an additional increment of about 25 percent. These increments are expected to permit construction and operation of the Compact Ignition Tokamak, resolution by the early 1990s of the central scientific feasibility question, and participation in the construction of an international engineering test reactor beginning in the mid- to late-1990s (see Chapter 5).

- The committee recommends that the Department of Energy develop a revised program plan providing for greater participation by U.S. companies, to ensure that these companies are not placed at a disadvantage relative to their foreign competitors. Such participation could, for example, follow the Japanese practice of assigning industry responsibility for both the design and construction of major systems and subsystems (see Chapter 5).

The CHAIRMAN. Dr. Dreyfus, let me interrupt you at that point and ask you a couple of questions. I have read your statement, and Senator McClure can hear further from that if he likes.

But first of all, I notice in speaking of the major constraints you talk about constraints on coal use for environmental considerations. Did you have in mind global warming?

Dr. DREYFUS. Yes. Primarily global warming. I think anything less than the CO_2 problem can be dealt with with technology at relatively modest economic penalties. So I am not talking about acid rain or that sort of thing.

The CHAIRMAN. I am interested to hear your view on global warming.

Dr. DREYFUS. My view on global warming? I think it is a very important political issue at this point and that there is likely to be action taken. The danger, I think, on global warming is that the action will be unwise and unplanned and precipitous.

The CHAIRMAN. Now, Dr. Dreyfus, would you explain to me when you say that successful commercialization of magnetic fusion energy is not——

Senator MCCLURE. Could I interrupt just a moment? I want to make sure that that statement is underscored on the basis of his past experience.

Dr. DREYFUS. That is true. Probably not as cosmic an experience as global warming, but useful nonetheless.

Senator MCCLURE. Thank you very much. And I do hope that people noted that. He is not a beginner in these matters, and he has observed some of these trends before. That is the reason I wanted to underscore the relevance of the comment, and the reason why perhaps there ought to be some attention to that comment.

The CHAIRMAN. Dr. Dreyfus, you say, "Successful commercialization of magnetic fusion energy is not expected until after the year 2050."

Now, when you speak of successful commercialization, are you talking about like full use of magnetic fusion or like the first operating commercial plant?

Dr. DREYFUS. There are two decisions points in this program. One, I think, will occur some place around 2020 when, if you look at the trends, it is likely that people will begin to think that in the next 10 or 15 years they need a new, large source of energy, and they will have to bet on one. At that point you would like to know whether fusion is a good bet.

The 2050 timeframe you would suppose that somebody has a prototype reactor——

The CHAIRMAN. I mean, that is the Shipping Port 1957 date, not the——

Dr. DREYFUS. It may be less than that. But by 2050 the decision point you are looking at is a commercial entity considering building the second generation commercial plant, an operating prototype that looks like a reactor and generates electricity.

The CHAIRMAN. Now, I have got to tell you, I find it amazing that in this day of high technology, it would take you 60 years to build a successful prototype for energy. And if nuclear fusion—nuclear fission is any teacher, I mean, Shipping Port I think was

1957. This is 1989. That is 32 years. So if you add that on, you would say that is 2080, almost a century from now until you could be making wide use. I do not know whether that is fair to use those kind of figures. But you are talking almost a hundred years before you are really doing that.

Now, you know, why would that be so long?

Dr. DREYFUS. Well, I think my colleague, Dr. Auer, is better equipped to tell you why the complexities of the fusion program would take that time; and I will ask him. But I would point out that where you are in this program is trying to prove scientific feasibility. You have not even approached engineering problems of any consequence in this program as yet.

Senator MCCLURE. Before turning to Dr. Auer, could I ask one question in that regard?

I think it is useful when we look at fission nuclear power that perhaps it is useful to recognize that the progress from 1957 to 1989 is not a straight line graph leading upward. It is perhaps fair to say that we are farther away from our goal in 1989 than we were in 1969. And perhaps that justifies some pessimism about our ability to do anything in a hundred years or any other period of time.

Excuse me, Dr. Auer.

STATEMENT OF DR. PETER L. AUER, PROFESSOR, DEPARTMENT OF MECHANICAL AND AEROSPACE ENGINEERING, CORNELL UNIVERSITY

Dr. AUER. Thank you, Senator.

I think Dan is taking a conservative point of view, and we were, just as Senator McClure has said, we were using very much the analogy with the nuclear fission experience. The notion is that by 2020 there is a reasonably good technological, engineering base from which to proceed with the commercialization effort; and over the period of the next 30 years, what will ensue will be one or more prototype plants which are not necessarily commercially competitive, but by the year 2050 or thereabouts the industry is prepared to put in front of utility executives plants which are fully competitive and are second generation plants.

[The prepared statement of Dr. Auer follows:]

Statement of Dr. Peter L. Auer

Professor

Department of Mechanical and Aerospace Engineering

Cornell University

Ithaca, New York

before

The Subcommittee on Energy Research and Development

of the

The Committee on Energy and Natural Resources

United States Senate

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a Hearing on

The Department of Energy's

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INTRODUCTION

Mr. Chairman, my name is Peter Auer. I am a Professor of Mechanical and Aerospace Engineering at Cornell University, and from September 1987 through the summer of 1988, I was a member of and ad hoc committee of the National Research Council, asked by the Department of Energy to look at the pacing of the U.S. Magnetic Fusion Program. I should like to thank you for allowing me to appear in front of your committee and to report on the results of our study.[1]

The previous speaker, Mr. Dan Dreyfus, has already mentioned that our committee viewed fusion as a potentially valuable energy source for the next century. Considering the various uncertainties faced by most energy sources known to us today, be they nuclear or non-nuclear, renewable or exhaustible, the prospect of commercially viable fusion reactors represent an important insurance policy to the Nation's and the World's energy future.

Having arrived at these preliminary conclusions, the committee then focused its attention on assessing the scientific and technical goals, objectives and performance of the Magnetic Fusion Energy Program, including a review of the program's research activities.

[1] "Pacing the U.S. Magnetic Fusion Program," prepared by the Committee on Magnetic Fusion in Energy Policy, National Academy Press, Washington, D.C. (1989)

One should note, however, that our committee did not perform an indepth technical review of the magnetic fusion effort, nor of any other potentially competitive energy technologies. It would take a more rigorous, systematic, comprehensive comparative assesment, than could be done within the limited scope of our study, to provide the information needed by those seeking to make rational R & D investment decisions.

GOAL OF THE U.S. MAGNETIC FUSION ENERGY PROGRAM

The stated goal of the U.S. magnetic fusion program is to establish the scientific and technological base required for fusion energy[2]. To accomplish this goal by early in the next century, a detailed technical plan was developed by DOE in an effort led by the Argonne National Laboratory.[3] As specified in the plan, four general technical issues must be resolved in order to achieve the program goal. These issues are as follows: magnetic plasma confinement, the properties of burning plasmas, materials for fusion systems and the nuclear technology of fusion systems.

SOME MAJOR PROGRAM ELEMENTS

Some of the major program elements that were brought to our committee's attention included the Compact Ignition Tokamak (CIT) and the International Thermonuclear Experimental Reactor (ITER). We, along with most other reviewers of the program, were persuaded that an essential next step in the program is to establish the properties of a burning plasma. The least expensive route, it appeared, is through the construction of a short pulse duration tokamak, the CIT, which would be fueled with a mixture of deuterium and tritium (D-T). Prior to the operation of CIT, valuable experience on the handling of D-T fuel could be gained by running the TFTR experiment at Princeton with tritium. These preliminary D-T burn experiments in TFTR were thus included as an integral part to the CIT program plan.

To develop the engineering and technological base for the design of fusion reactors, however, it will be essential to construct a long pulse duration tokamak. This would then provide an engineering test reactor (ETR) suitable for studying the behavior of candidate materials and reactor components in an environment similar to that which would be encountered in full scale fusion reactors.

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2 "DOE Magnetic Fusion Program Plan," DOE/ER-0214 (February, 1985).
3 "DOE Technical Planning Activity," Final Report ANL/FPP-87-1 (1987).

The ITER study which is proceeding at this time is a joint, international study looking at the preliminary design of an ETR. Our committee noted that in Europe, Japan and the U.S.S.R. there are studies concerned with the design of national ETR's as well, as possible hedges in the event that international agreement on the joint construction of an ITER is not achieved. The U.S. program, however, lacks such a backup.

A potentially larger void in the U.S. program is the absence of detailed plans for the commercialization of fusion. It will require considerable time and federal resources to help U.S. industries develop the requisite technical and scientific breadth and depth necessary for them to be willing to make significant investments in fusion. Moving fusion from scientific feasibility through engineering feasibility to commercial feasibility would now be more difficult in the United States than abroad. Unlike the private sector in Japan and, to a lesser extent, in Europe, the U.S. private sector has not participated to a great extent in the design, construction, and operation of experimental fusion systems. In the United States, with the exception of General Atomics, this activity has been almost totally controlled and executed by universities and national laboratories. Consequently, there has not yet been a real opportunity to build a U.S. industrial cadre in fusion R&D. Creating such a cadre could prepare U.S. industry for future participation in commercial fusion development.

I should like to emphasize that it is not too early to move vigorously on a fusion energy program if we are to have an effective commercial product in hand by the middle of the next century. If the scientific and technological base that the DOE has promised can be obtained in the time frame of the years 2010 to 2020, it will probably take another 20 to 30 years to complete a successful commercialization effort. With these thoughts in mind, I should like to move on to the findings, conclusions and recommendations of our committee. To begin with, however, allow me to list the four different funding level cases that were considered.

IMPLICATIONS OF VARIOUS FUNDING LEVELS

Funding levels set boundaries on the content of the MFE program and, in turn, the extent of activities that can be supported by the program determine its rate of progress toward meeting its objectives. From among the possible variations of funding levels as

they affect program priority and pace, the committee selected four budget alternatives and analysed their likely impact on program scope and performance.

Alternative 1: A broad-based, relatively independent U.S. fusion research effort to foster continuing improvement of the tokamak and to develop other confinement concepts to a level that permits comparison of the alternative approaches. Major elements of such a program would include the ignition and short-duration burn experiment (CIT), a domestic ETR, and engineering development activities leading to the commercialization of fusion energy. If successful, a balanced, comprehensive program with commercialization as its ultimate goal would ensure the availability of economic fusion power by as early as the mid-21st century, when new sources of electrical power are likely to be needed.

This approach would permit DOE to accomplish its scientific and technical objectives by about 2010, reduce the risk that commercial fusion reactors will be based on a less-than-optimal confinement configuration, and ensure that the United States is a major beneficiary of the direct and indirect technological and economic benefits of magnetic fusion energy.

A magnetic fusion program of this kind would require sizable increases compared to the Fiscal Year 1989 budget, which is about $360 million. An increase (measured in constant dollars) of 10-20 percent would be required soon, to about $(1989) 400 million, with a subsequent increase to about $(1989) 750 million in the mid-1990s, when construction would begin on a domestic ETR.

Alternative 2: Increase the current fusion research and development budget (measured in constant dollars) by 10-20 percent, to about $(1989) 400 million, and have it remain at that annual level until 1992, then increase it by another 25 percent, to about $(1989) 500 million to provide the U.S. share for ITER construction. This funding would permit DOE to accomplish its planned objectives within the international collaborative program by about 2010. At this funding level, DOE could proceed with CIT, participate in the construction and operation of ITER, and maintain the present balance of its program, including the investigation of alternate confinement concepts, although on a scale much smaller than what would be possible under the first alternative. This funding option would not permit DOE to add commercial development activities to the program until after the 2010 time period.

Alternative 3: Maintaining present funding levels (measured in constant dollars) and remaining within the international collaborative effort. This option will force the fusion program to eliminate some elements. Proceeding with CIT would mean that its construction costs would have to be absorbed by other R&D activities. Eliminating the D-T fuel experiments in TFTR would provide some cost savings, but at the possible expense of jeopardizing the success of CIT. At an estimated annual cost of $100-$150 million between 1992 and 2005, U.S. participation in ITER would not be an option at the current funding level. To finance participation in the proposed international collaboration, the program would have to reduce domestic research projects to a level that would prevent the United States from being a viable partner and preclude it from benefiting fully from the findings of ITER-related research.

Another course of action with a constant budget would be to defer or eliminate CIT. Resources allocated for this experiment could be redirected into a more exploratory, general physics research effort on tokamaks and alternative confinement concepts. Impacts and risks of such a decision were outlined above.

Regardless of the specific changes in program content, maintaining funding at the current level in real terms would not permit DOE to accomplish its objectives by 2010. Program capabilities would diminish, and it is likely that fusion-generated electricity would not be an energy option that could result from the U.S. effort until after the mid-21st century.

Alternative 4: Decrease funding below the present FY89 level. Given the constraints already imposed by the fusion program's current budget, a decrease in funding would result in a major retrenchment in U.S. fusion research and development efforts. CIT and ITER would not be options, nor would any other major U.S. fusion experiment. Resources could be concentrated on basic research on plasma physics and, to some degree, on confinement concepts. The potential importance of this research, however, would be limited, because the values of plasma parameters would not be in reactor-relevant regimes, with the possible exception of D-T in TFTR.

The reduced program capability, including the loss of science and engineering personnel, would greatly constrain U.S. opportunities for collaborative research with the world's major fusion programs. Decreased funding for three to five years would dilute the program to the point that restoring research and development activity to its original depth

and breadth could take a decade or more. It would take a comparable amount of time to restore confidence in the United States as a partner in international fusion projects.

FINDINGS

1. DOE plans include two major experiments that will be instrumental in demonstrating the scientific and engineering feasibility of fusion by 2010. One, CIT, is an intermediate, scientific experiment that will permit research on the ignition and short-duration burn of plasma. The other, an Engineering Test Reactor, is an engineering facility of larger scale and will investigate the physics of long-duration burn and will address materials and technological issues.

2. The U.S. has proposed an International Thermonuclear Experimental Reactor, to be jointly constructed and operated by the world's major fusion programs.

3. Although they are participating in preparatory stages of ITER, the European community, Japan, and, presumably, the Soviet Union are also proceeding with plans for their own ETRs. The U.S. does not have a comparable planning effort for a domestic ETR.

CONCLUSIONS

1. Decreasing support for the MFE program will necessitate further retrenchment, reducing U.S. fusion research and development to below a critical level. DOE would not accomplish its scientific and technical objectives by 2010. Diminished capability would also make the U.S. a less desirable partner in international collaborations.

2. Continuing support at the present level would force the restructuring of fusion program plans. The D-T fuel experiments in TFTR could be foregone. CIT could be eliminated, delaying ignition and burn studies until an ETR is built. These cost-saving measures would significantly increase the risk that an ETR may achieve its objective. Building a CIT without increased funding would require cancellation of other program elements and probably necessitate international collaboration in pooling manpower and resources of that project.

3. DOE's participation in the conceptual design of ITER is both desirable and appropriate, but construction and operation of the proposed device will require unprecedented level of international cooperation and coordination. The annual costs to the U.S. to participate in the ITER project are estimated to be about one-third of the annual costs of a completely domestic ETR project.

4. U.S. participation in ITER should be crafted so as to ensure that the nation will have capability to proceed on its own to the subsequent commercialization phases of fusion development.

5. ITER and other collaborative projects that may evolve should complement and supplement domestic fusion research programs. Reducing the present level of domestic fusion research would undermine the potential benefits of collaboration.

6. If the U.S. does not participate as an equal partner in international projects, its potential competitive position in anticipated world markets for fusion energy and fusion-related technology will be jeopardized.

7. A domestic ETR offers several advantages, although it would require as much as three times as much in annual funding during its construction period compared to the cost of U.S. participation in an ITER project. However, in a domestic ETR, the U.S. would control key programmatic decisions.

RECOMMENDATIONS

1. Committee recommends that the U.S. enter into partnership arrangements for the next major steps in an international fusion program as the most cost-beneficial approach to fusion over the next decade.

2. Committee recommends an increase over current fusion program funding of about 20%, held steady for the next five years, followed in the mid to late 1990's by an added increment of about 25%. These increments are expected to permit construction and operation of the Compact Ignition Tokamak (CIT), resolution by the early 1990s of the central scientific feasibility question, and participation in construction of an ITER beginning mid-to-late 1990s.

3. Committee recommends that the DOE develop a revised program plan providing for greater participation by U.S. companies, to ensure that these companies are not placed at a disadvantage relative to their foreign competitors. Such participation could, for example, follow the Japanese practice of assigning industry responsibility for both the design and construction of major systems and subsystems.

The CHAIRMAN. Well, that still does not satisfy me as to why it takes until 2050 to get this first plant. I mean, if we can get break even and have demonstrated scientific feasibility, then I would think we would—unless there is some economics that would clearly be against it, I would think the country would commit itself in a very big way to developing commercial fusion. And we would not be spending a few hundred million dollars a year; we would be spending billions, I would think, once you get scientific feasibility.

Is that what you are assuming, that once you get the scientific feasibility, you continue to fund it at a very low level?

Dr. AUER. No, no, no. 2020 is not scientific feasibility, Senator Johnston. No. This is, say, a Shipping Port point where you have built a small prototype by 2020, and we are talking about going from Shipping Port to a modern 1,000 megawatt reactor. And that, in the nuclear industry, took some period of years, and we were being conservative and arguing that perhaps it will take a few decades after your Shipping Port demonstration before you have an economically competitive plant that is on line—on line by 2050, not under construction, but on line and generating power. That is not necessarily one plant, but it is several plants.

Dr. DREYFUS. The sort of answers that I think would be required to spur some sort of a major commitment, major increase in activity towards commercialization are not going to be the result of a CIT. They will come from the next experiment, possibly from the international test reactor, but not the CIT, even if it is successful. So you are looking at the timeframe of the international test reactor before, I think, you would have much more confidence than you have right now.

The CHAIRMAN. Well, what is the relationship between the international test reactor and CIT?

Dr. AUER. The CIT is basically a scientific experiment. It is the last important scientific experiment for magnetic fusion. It will prove not only that plasma can be ignited, but that it can be magnetically confined under an ignited condition. But it is a short duration machine; it does not provide the environment that is adequate for testing all of the various components that will have to go into a fusion reactor.

The CHAIRMAN. But is it a necessary building block to ITER?

Dr. AUER. I think that in order to have confidence in a multibillion dollar facility such as ITER, I think that it is prudent to have a preliminary step. Whether it is CIT or CITX, that is not for me to say; but I should think that one would like to have confidence in understanding the nature of an ignited plasma, how to confine that magnetically, how it behaves under magnetic confinement, before one is willing to spend several billions of dollars on an ITER type of facility.

The CHAIRMAN. And, conversely, you should not proceed with ITER unless you have a CIT?

Dr. AUER. I would say so.

The CHAIRMAN. Would you agree with that, Dr. Dreyfus?

Dr. DREYFUS. Well, the only thing that I can draw from is our ostensible partners all will be doing some sort of an intermediate experiment. They, if I understand the situation right—and Peter will stop me if I do not—the Europeans, the Japanese, and prob-

ably the Russians will build an engineering test reactor of somewhat more consequence than the CIT before they enter into the final design and construction of the joint reactor. So there seems to be a community of belief that you have to do something else, or that you should do something else first.

The CHAIRMAN. By the way, can you tell me what that Soviet reactor is designed to prove, the one that has that quick pulse of——

Dr. AUER. Is that the T15 that you are referring to?

The CHAIRMAN. Yes.

Dr. AUER. The one that I know a little bit about is a superconducting—that is, it has superconducting magnets, just as the one in France, I believe.

The CHAIRMAN. No. I am talking about the Soviet reactor.

Dr. AUER. I said just as the one in France, so the one in the Soviet Union has superconducting magnets. We have not built any tokamaks with superconducting magnets so far.

The CHAIRMAN. Do you know what the Soviet is designed to prove? Is that designed to get break even?

Dr. AUER. I do not know about that, sir.

The CHAIRMAN. Or ignition, should I say.

Dr. AUER. No, no, no. I am rather certain that it is not meant to prove ignition.

The CHAIRMAN. Dr. Dreyfus, what is your view of inertial confinement as magnetic competition?

Dr. DREYFUS. Well, they are all programmatic, not technical, because my only knowledge of the weapons program is outdated, as my security clearance is. But I think there are a couple of things that should be considered in dealing with that.

One is that the current weapons program is a very critical program on the defense side, as you probably well know. Nothing should be let get in the way of that. I think it gets more important with every passing day because of things that are going on in the world.

It is difficult for me to imagine taking that weapons program, and dealing with it as either an energy program or particularly an international, cooperative notion of how to get a reactor in place. So I think there are some serious difficulties in marrying a civilian energy program and a weapons program; and I would caution about that.

We, in the committee, did talk a little bit about it, though it was beyond on our scope, and recognized that the inertial confinement program is the other thing that is going on today that is funded and that deals with a scientific effort that has relevance here. As I understand the weapons program, nobody is spending very much time or any money to look at conceptual designs for transferring that knowledge into the energy field; and it might be well they do that. But my own feeling would be they should do it behind a fence. And, of course, our committee did not look at it all.

It is not clear to me how you can draw on that highly classified program and then do the energy-related work outside in a community that now exists for magnetic fusion energy. That is going to be a very difficult thing.

The CHAIRMAN. Well, you know, there may be a lot of physics uncertainty in these programs, but there is a lot of budget uncer-

tainty in the next few years. There is also some budget certainty, and the budget certainty is that dollars are going to be scarce. The uncertainty is we just do not know how scarce.

Now, my concern is that there is not enough money to do a competition. I mean, I, at least mentally in my own mind, abandoned the idea of a competition about the time DOE dropped ICF.

Dr. DREYFUS. I find it difficult to visualize a competition between these two programs, and I do not know what forum this competition could take place in, because of the relatively significant classified nature of our inertial confinement program. I cannot visualize it.

I can see that there is something going on in the weapons program that people ought to remember as a potential fallback, but I do not know how you bring them both in to something like a parallel program.

The CHAIRMAN. Well, I mean, when you talk competition, you usually talk in terms of putting a lot of money in both so that you have demonstrated at least the scientific feasibility or nonfeasibility of both, which contemplates big revenue streams in both.

Dr. DREYFUS. Well, there are big revenue streams in both, and they are all attacking various scientific problems. What I have no knowledge of is whether the scientific problems relevant to the weapons program are at all relevant to an energy application.

And what might be worthwhile is for some duly cleared group of experts to look at that. I do not know. We did not look at it.

The CHAIRMAN. Well, Dr. Hunter said that he foresaw separate parallel courses for ICF, one weapons and one civilian.

Now, I do not know whether enough attention has been given. I do not think he is giving that as a definitive——

Dr. DREYFUS. I see organizational problems with that, not scientific problems.

The CHAIRMAN. I have seen the weapons laser deal. It seemed to me that an improved laser is an improved laser, whether used on a weapons target or on an energy target.

Dr. DREYFUS. Do you have anything to say about this, Peter?

What I think is lacking in the weapons program is any appreciable effort to try to relate the concept to the generation of electricity for commercial purposes.

Now, there is bound to be a body of classified knowledge existing. And I can see that it might be fruitful for somebody to explore, at least conceptually, how you would turn that into some sort of a reactor.

What I do not see is an open competition involving the tremendous amount of international and general scientific activity that goes on in magnetic fusion being extended over into that other program. That seems to me to defy reality, and that is why my skepticism exists.

The CHAIRMAN. Dr. Auer, what is your view on that?

Dr. AUER. Senator Johnston, I have always had a very warm regard for inertial confinement fusion. About 10 years ago, the Office of Inertial Confinement asked me to review the program with a view to seeing whether it had energy applications. And I think you are well aware of what has happened in the intervening 10 years with regard to that matter.

I am very surprised by the testimony that Dr. Hunter presented, that the Department is seriously considering promoting it as a energy program now, once again. I would welcome it if I thought that this had high credibility.

Dr. Hunter has put his fingers, certainly, on the problem that has plagued the energy applications all along. Namely, whether you could develop an economically attractive driver with suitable requirements for energy applications.

And I grant that that could be carried out, I suppose, somewhat outside of the classification fence that surrounds the ICF program. But the rest of the program is surrounded by classification issues.

To develop ICF into a credible civilian energy program, somehow one would have to get rid of those classification barriers. And I would not be tempted to speculate whether that could be done and how easily that can be done, or whether you want to do it at all.

The CHAIRMAN. I do not understand why classification barriers prevent your using a technology for energy.

Dr. AUER. Well, because the very nature of the interaction between the laser and the target and the design of the target is, at the moment, classified. I think it would be difficult for a large enough scientific and technical body to become involved in this program without all of them having full access to the classified material. If that is the intent, fine. But, then, the very nature of the program would always remain classified.

It would be virtually impossible for us to share our knowledge with our international partners and to ask them to collaborate with us in the same sense that we are doing in the magnetic fusion area.

The CHAIRMAN. Let me ask you one final and key question here. And, Senator Bradley, I might say that the predicate for this question is that Dr. Hunter has testified, and also the two experts have testified, that in their view, there are physics questions yet to be answered on CIT; that their recommendation is to pursue the physics for another three to four years before beginning to build CIT.

As an ancillary thing, Dr. Hunter said that he thought that we should put $50 million additional into inertial confinement fusion and have a parallel track for the two programs and, in effect, a competition, which are the questions I have just been asking.

But this final question is: What is the advisability of delaying the CIT for three to four years, as opposed to building it at this time?

Dr. Auer?

Dr. AUER. Well, I do not think I have an independent view on that. You have heard testimony from representatives of the magnetic fusion community, the Magnetic Fusion Advisory Committee; you will hear from people from Princeton later on. As far as our committee is concerned, we very much support the idea of an ignition experiment. We would like to see an ignition experiment conducted as soon as feasible in order to get on with the energy development phase of the program.

The question of risk, technical risk or scientific risk involved, is actually beyond the competence of the committee which I am representing.

The CHAIRMAN. Dr. Dreyfus?

Dr. DREYFUS. Well, again, our committee felt that the CIT was the set piece, and therefore our recommendations all center around the notion of getting the CIT constructed in a timely fashion.

Personally, I would only make two observations about it. One, is we are embarked, like it or not, and pretty well along in a big international exercise, which contemplates the partners going along with their programs as originally envisioned. I would hope that if somebody now revises the U.S. program, it does not in some way diminish our ability to get our money's worth on this international exercise. If we somehow do things now which put us a step out of phase with our partners, we are likely to spend the same amount of money on the international program, but get much less value than do the Japanese and the Europeans and the Russians.

So I hope that all of this, if it goes forward, any major revisions in this program——

The CHAIRMAN. All of this, you mean ITER?

Dr. DREYFUS. I mean if, in fact, we revise the existing program by changing the time frame for the CIT and shifting money into other not-now-contemplated activities, that the pace of our work is consistent with what we presumed when we entered into this international arrangement.

The CHAIRMAN. Well, as you advised before, we ought to have CIT before ITER. It is a necessary building block.

Dr. DREYFUS. But we need to have it at the right pace, otherwise if we do not do what we said we were going to do, we will become a strange international partner. The result of that will be that we will probably not get our fair share of the results. We will be less prepared to enter into the design——

The CHAIRMAN. Well, if we do delay CIT for three to four years, then we ought to have a corresponding delay in ITER. And if we do not have a corresponding delay, we should not spend the billions on it. Would that be fair?

Dr. DREYFUS. Well, that is one possibility. The other of course, is that the partners go along without us. They all have their own independent domestic programs. We may be left behind, or else we may remain involved without getting full participation.

All I am cautioning is that if we redesign the program, they do not forget about the fact that we have already committed to march in step with a lot of people, and this would break step. I think that is an important consideration, because a lot of the money and a lot of the ability to go to the next experiment is based on international funding. That is about maybe a third of what we would have to fund if we did it alone, so we cannot very well forget that we have done that.

The CHAIRMAN. Thank you.

Senator Bradley.

Senator BRADLEY. Mr. Chairman, thank you very much.

I am sorry that I missed the testimony of Assistant Secretary Hunter. I would like if it would be all right with you to submit some questions for the record for him to answer, if that is possible.

The CHAIRMAN. Of course.

Senator BRADLEY. From what I have learned just in coming in, he said, I am troubled by it, and I take it from what Dr. Dreyfus

said, you were on the Magnetic Fusion Energy Advisory Committee?

Dr. DREYFUS. No. We are representing an Academy of Sciences committee, which just concluded a study of the pace and direction of the program.

Senator BRADLEY. Because one of the things that puzzles me is how Mr. Hunter could come in recommending a dramatic cutback in——

The CHAIRMAN. Delay really.

Senator BRADLEY. Or delay, essentially. Someone once said the power to postpone is the power to negate. And that is an interpretation, particularly since the advisory committee in the Department itself urged that the Office of Fusion Energy proceed with plans for building the CIT. And also the National Research Council and the National Academy of Sciences recommended that the Federal Government increase its financial support for magnetic fusion energy research by 20 percent.

And so there are a lot of questions here that I have. I think that the witnesses on panel 3 will be better able to deal with a lot of the complexities of the program from their point of view.

But, I have some real concerns about delaying, not the least of which are the points that you just made, Dr. Dreyfus. I mean, we are part of an overall international effort. And when we stretch out, we delay, we lose leadership, we lose opportunity. And I would hope that we would go slowly on this process. And I would like Mr. Hunter to answer a whole series of very specific questions, not the least of which are the scientific bases for his decision and what his specific expectations are, and where he would specifically like to see funds go, and to what projects, and to what aspects of those projects. Because I think that the information that we derive from his answers would help the committee in its own deliberations greatly.

The CHAIRMAN. Thank you.

Gentlemen, thank you very much. We appreciate your testimony. Glad to have you back, Dan. Come back more often.

Next, we will hear from panel 3. Let me say while Dr. Harold Furth from Princeton and Dr. David Overskei also from GA in San Diego, while they are on their way up, let me apologize for having to leave early, in about five minutes.

Gentlemen, for my own edification, I will let Senator Bradley—can you stay?

Senator BRADLEY. I cannot, Mr. Chairman. I am sorry I cannot stay longer than another three or four minutes.

The CHAIRMAN. All right. If I could get you all to summarize in just a couple of minutes your main proposals and perhaps we can let—I had hoped that Senator Ford would be able to be here. But the supplemental appropriations conference is coming, and I have simply got to be there.

Why don't we do this, let me ask you just a couple of questions, then adjourn the official hearing, but Dr. Cooper can ask questions which we will ask the reporter to take on. It will be an unofficial hearing, but it might be more sensible than if I asked the questions anyway.

So, please give us your reaction to Dr. Hunter's advice.

Who wishes to be first?

STATEMENT OF DR. HAROLD P. FURTH, DIRECTOR, PRINCETON PLASMA PHYSICS LABORATORY

Dr. FURTH. Could I, to use my time fruitfully, set a couple of things straight that were raised and not gotten quite straight?

The CHAIRMAN. Yes, indeed.

Dr. FURTH. Concerning the testimony of the experts about ignition and CIT, Dr. Molvig, speaking for his sub-panel said one thing, namely, sit and wait. Dr. Baldwin, speaking for the real Magnetic Fusion Advisory Committee, said what he quoted, namely, do not sit and wait; do something. And it says rather specifically, "We believe that progress and the remaining transport questions will be adequate within the next few years to provide a basis for ignition; that the CIT design, R&D and prototyping should proceed in parallel with the transport work. And that the checkpoint should release construction funds for the project, rather than initiate them."

That says adhere to the present schedule; do not put it back three or four years. And as for this phase 1, phase 2 thing, the important thing that was not said was to raise the capability from phase 1 to phase 2, what is missing is $140 million worth of power supplies and nothing else. No factor of two, no change in the basic machine.

And as for our knowledge being inadequate now to design the basic machine, I think that is rather misleading because the gross components, the coils, the vacuum chamber, things of that sort, have been shoved and hauled around a centimeter at a time for the last five years, and they are not going to change very much.

What we are going to learn from transport studies is sophistication in the inside, furbishing of the machine, and the diverter and the tiles and the heating methods, things of that sort. And that is precisely the sort of thing which could come along during the next few years and teach us to optimize the machine.

Now, concerning Dr. Hunter. Let me maybe—do you still have one minute or three minutes?

Let me maybe make a point. And that point can perhaps be made from the point of view of what is the rest of the world—how do we relate to the rest of the world? We are not working in a vacuum, as Mr. Dreyfus pointed out. You know, we are in the midst of a community. And what are they planning to do and how do we relate to it?

Now, what they are planning to do is to proceed with the most promising line, the tokamak line, because in the next generation of machine, that can lead to a device which can produce peak fusion powers like 1,000 megawatts. It can do it for long pulses; it can over extended periods of times produce 100 megawatts. It can, in the next generation of machine, get us into a real fusion reactor, rather than various kinds of physics toys.

And so, their view is they are just going to do that. They are going to keep an eye on ultimate concepts. They are going to keep an eye on inertial confinement. But they are not going to say let us slow down this thing which is on the way to a real reactor; let everyone else catch up so that we can play games.

The CHAIRMAN. In a word, do you have minimal high regard for ICF?

Dr. FURTH. I am sorry?

The CHAIRMAN. In a word, do you have minimal high regard for the ICF?

Dr. FURTH. I am not speaking of myself.

The CHAIRMAN. Yes.

Dr. FURTH. ICF people are among my best friends and ex-students. I love them; they do nice work. I was saying what the Europeans think, and the Japanese and the Russians.

The CHAIRMAN. You do not want to talk about your friends.

Dr. FURTH. Their view is you have got something that works; it is going to make the first real reactor; do not get yourself distracted and say oh, for heaven's sake, let us hold it back so all the other things can catch up.

Now, their plan is either to get altogether with us and build the ITER device, which we plan to start construction in 1995 and maybe finish construction a little after the year 2000 or, if that falls apart because of people like us, the Europeans are prepared to go it alone with the NET; the Japanese with the FER; the Russians with the OTR. They are not going to hold their breath while we do peculiar things.

And that brings me to my final topic, if you will still tolerate my using your time. You know, what does the rest of the world think of us? I think that is worth contemplating. They think we are full of surprises. For example, in the late 1970s they were very surprised that all of a sudden the United States was ready to build an engineering test reactor all by itself—start building it in 1982 and have it running by 1990—the same sort of object that we now call the ITER.

They were equally surprised when in the early 1980s we dumped the idea completely. Suddenly, no engineering test reactor, not even the engineering test reactors which are now designed in Europe, Japan and the Soviet Union. We just pulled out.

Then in the mid-1980s, they were again surprised by the United States saying we do not know enough physics to do an engineering test reactor. You know, was it supposed to ignite? And we do not know that.

The CHAIRMAN. Well, I understand this argument about the uncertainty about what people think about us, but what do you think about Dr. Hunter's statement that the probability of getting ignition is very low?

Dr. FURTH. Well, let me put that into an intellectual context for you. If you say to Leon Lederman what is the probability that you will find fascinating new particles that will justify the investment? You know, what is he going to say? He is going to say obviously that is what I personally very strongly believe and why this machine is sized as it is sized. And yes, I expect this probability is good.

And then you say, but can you prove it? And then you have got him. Because either he will say no, I cannot prove it, or he will say oh, yes, I can prove it. But the moment he says that, then it follows that you should not be building this machine because you already know the answer, you should be building the next machine.

And that is exactly what the Europeans and the Japanese do to us on the CIT.

The CHAIRMAN. Well, what do you think about the CIT?

Dr. FURTH. In my opinion, it is the right size to ignite. In my opinion, we can get it to ignite through cleverness in the first embodiment. The quality of confinement needs to be no better than that now shown in TFTR. If we achieve even half the quality of confinement we now have in TFTR this thing will ignite in the second phase for another $140 million.

[The prepared statement of Dr. Furth follows:]

Statement of Harold P. Furth
Director, Princeton Plasma Physics Laboratory
before the
Subcommittee on Energy Research and Development
Senate Committee on Energy and Natural Resources
14 June 1989

Mr. Chairman, Members of the Committee:

I very much appreciate the opportunity to testify to you concerning the state of the U.S. magnetic fusion program. For the sake of brevity, I should like to limit my testimony to six general points, dealing with the motivation for fusion, the current technical progress of the U.S. fusion program, and the value of a well-considered long-term plan for U.S. fusion research.

1. Need for Fusion Energy

The report of the National Research Council Committee on Magnetic Fusion draws attention to growing uncertainties about the world energy supply during the next century. The report notes that coal and fission fuels will be available, but their use may be subject to "constraints" relating to environmental issues.

For an environmentally superior new energy source such as fusion to begin to make an appreciable contribution to the U.S. base-load electric power plant would require a lead time of at least 30 years. A solid technical basis for planning the introduction of a new energy source would therefore have to be available not long after the turn of the century.

Considering the enormous sums that will be at stake in making the right decisions about world energy supply, the available research time is all too short. The next two decades must clearly be put to efficient use. As the National Research Council report points out, even with an exceptionally high degree of reliance on international collaboration, funding for the U.S. magnetic fusion program will need to grow at least moderately during the 1990's.

2. Public Support for Fusion Research

Ten years ago, when public awareness of the underlying energy problem was suddenly aroused by a near-term dislocation of fossil-fuel supplies, there was support for a magnetic fusion program at more than twice the present level (in constant dollars). Meanwhile, the potential drawbacks of the available energy sources have been dramatized by some large-scale environmental disasters, as well as by the observation of global environmental effects, and there is increased appreciation of fusion as an environmentally acceptable new energy source. I believe that the public

would be likely to applaud the leadership of the Congress and the Administration in establishing a realistic, sustained effort to make the fusion option available to the country as part of our insurance of a stable long-term energy supply.

3. Progress in Plasma Physics

At the outset of modern fusion research, there were three main areas where fundamental new physics insights had to be developed: (1) the gross stability of a body of plasma when confined in a "magnetic bottle", (2) the heating of plasma to reactor temperatures (hundreds of millions of degrees centigrade), and (3) the use of magnetic fields to insulate the plasma against losing heat to its cold surroundings.

In regards to the plasma stability problem (1), fusion researchers have already achieved highly sophisticated levels of theoretical understanding, as well as reactorlike experimental performance. Now that full reactor temperatures (300 million degrees) have been reached in the Tokamak Fusion Test Reactor (TFTR), the plasma-heating problem (2) can be regarded as essentially solved.

The third field of research -- plasma transport -- presents some of the most complex and challenging problems in all of modern physics, but even here substantial progress has been made. The basic physical mechanisms of plasma heat conduction are largely understood -- though their full nonlinear effects cannot yet be derived theoretically from first principles. Reliable predictions concerning the performance of next-step experiments are typically based on a combination of general theory and the projection of observed experimental trends (i.e., empirical scaling laws).

4. Performance Predictions for TFTR and CIT

The key physics issues in designing TFTR were: (1) confinement of energetic ions (which are responsible for most of the fusion reactions) and (2) confinement of the bulk plasma. In both regards, the predictions made by the U.S. fusion community on the basis of the data pool available in 1976 have turned out to be accurate.

TFTR has already achieved a plasma heat-confinement parameter ($Tn\tau_E = 4 \times 10^{20}$ keV cm^{-3} sec) that exceeds the 1976 expectations, and the attainment of actual break-even conditions (where the fusion power generated by a deuterium-tritium plasma is equal to the input power required to keep the plasma hot), is now only a factor of two away (cf. Fig. 1). As the TFTR plasma current and heating power are raised toward their full values, we expect to achieve break-even conditions -- reaching the top of the range of TFTR project objectives defined in 1976 (i.e., the range from one-tenth of break-even to full break-even).

The objective of the Compact Ignition Tokamak (CIT) is to ignite a deuterium-tritium plasma -- producing hundreds of megawatts of fusion power output without any requirement for plasma-heating power input. The key physics issues in designing CIT are: (1) whether the alpha particles (energetic helium nuclei) generated by the deuterium-tritium fusion

reactions will be well confined so that they will serve to heat the plasma and (2) whether empirical projections of bulk-plasma heat confinement will again prove accurate.

Based on our experience with TFTR and considering the enormous growth, since 1976, of our confinement-physics understanding and empirical tokamak data base, there is reason for substantial confidence in the achievement of ignition in CIT. There are also important remaining uncertainties of CIT physics. The U.S. fusion community has led the way in pointing out these uncertainties and stressing the need to resolve them by means of an exploratory ignition-physics project (CIT), prior to plasma-burning operation in the International Thermonuclear Experimental Reactor (ITER). If the CIT physics performance were to become "too predictable", our partners in fusion research would conclude that the CIT mission was no longer a useful intermediate step in the world program.

In the judgment of the U.S. fusion community, as expressed by the letter of March 9, 1989 from the Magnetic Fusion Advisory Committee to Dr. Robert O. Hunter, Jr., Director of Energy Research, the outlook for CIT may be summarized as follows:

> "In spite of the uncertainty in predicting ignition, the Panel [MFAC Panel 22] endorses the CIT concept as the proper next step for the program; and, as the general parameter range for the machine is known, it endorses continuation of the necessary design and R&D for the machine. MFAC likewise continues its support for the construction of CIT as expressed on a number of earlier occasions. This will require additional funds in future years."

5. Developing a Superior Fusion Reactor Design

Great progress has been made since the initial Soviet tokamak breakthrough of the late 1960's, which established for the first time that a magnetic fusion reactor of practical size should be achievable. Confinement projections now show that the reactor size can be further reduced. Recent experiments with tokamaks of special design, such as the PBX-M at Princeton and the DIII-D at General Atomics, show also that the plasma pressure (beta value) can be further increased. The promising implication is that reactors can be built in smaller physical size and at correspondingly lower cost, without reducing the same fusion power output rating. Special techniques have also been developed for maintaining the tokamak reactor plasma in steady-state operation, rather than in the pulsed-burn mode that had earlier been considered inherent in the tokamak approach.

A number of speculative and highly promising new ideas for further improvements in reactor performance are being explored in parallel with the major program thrusts toward break-even and ignition -- which must be designed to maximize predictability by relying only on well-proven techniques.

6. Planning for the Future

The progress of U.S. magnetic fusion research has been hampered by a lack of long-term consistency in the setting of official program objectives and funding levels. Reviewing the historical record, one finds that five-year periods of intense optimism and programmatic effort have tended to alternate with fifteen-year periods of pessimism and programmatic stagnation. A steadier long-term planning and funding process would be likely to result in improved cost-effectiveness and productivity.

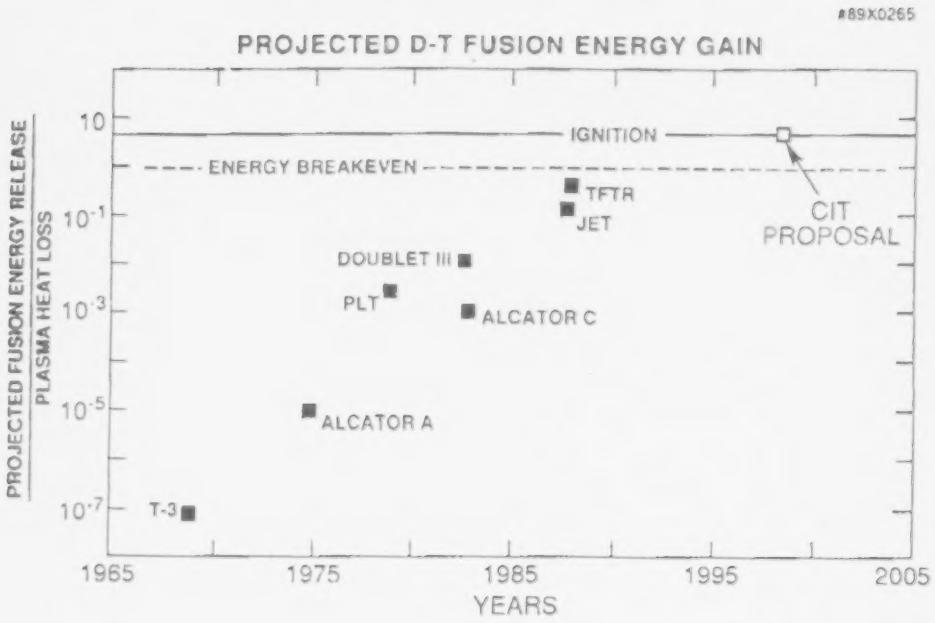
The NRC report makes some useful recommendations along this line, and also points to the stabilizing influence of U.S. collaboration with the fusion programs of Europe, Japan, and the USSR, which have followed a more predictable long-term course. The magnetic fusion community looks to the Congressionally mandated 1989 ERAB review of our program as an appropriate forum for us to present a cost-effective and timely plan for attaining the fusion goal.

Meanwhile, the vitality and potential of the present program need to be maintained -- particularly the design and R&D efforts for the CIT project. Proceeding vigorously on the CIT project schedule would lead to the achievement of ignition around the year 2000 -- in time to guide the planning for long-pulse fusion-power production in the internationally proposed engineering test reactor project ITER. Timely initiation of CIT construction is needed to seize an exceptional opportunity for restoring U.S. leadership of world fusion research while advancing cost-effectively towards the common fusion-energy goal.

Following an informal meeting of fusion scientists with Secretary Watkins, on April 17, the Magnetic Fusion Advisory Committee voted at its meeting of June 7 to provide the following summary statement of the fusion community position on CIT:

> "...Secretary Watkins expressed his support for an ignition experiment within ten years and the need for a physics checkpoint on the transport issue. In response to the sentiments he expressed, we believe that progress on the remaining transport questions will be adequate within the next few years to provide a basis for ignition; that the CIT design, R&D, and prototyping should proceed in parallel with the transport work; and that the check point should release construction funds for the project, rather than initiate them."

In conclusion, I would like to thank the Committee for its long-standing support of U.S. fusion research. We have now entered a period where high-leverage technical results are being obtained, great opportunities for progress are opening up -- and far-reaching programmatic decisions need to be made. We look to your continuing support and guidance for the coming decade.



A basic measure of progress in fusion research is the ratio of the fusion energy release to the heat loss from the burning plasma. This ratio can be projected to deuterium-tritium (D-T) reactor plasmas from the present-day experimental data on deuterium plasmas. The T-3 tokamak at the Kurchatov Institute in Moscow made the first major break-through in 1968. The Alcator A and C tokamaks at MIT, the PLT at Princeton, and the Doublet III at General Atomics extended this progress during subsequent years. The TFTR at Princeton and the European JET device at Culham, England are now approaching the break-even point. When the ratio of fusion energy release to plasma heat loss equals five, the plasma heating due to the energetic helium nuclei produced by the D-T reaction is sufficient to maintain the plasma temperature without need for any external heat input. This plasma regime is called "ignition." The proposed CIT device is currently scheduled to start experimental operations in 1997. With sufficiently strong support, the CIT project could attain ignition by the end of the 1990's.

The CHAIRMAN. Dr. Overskei, what do you think?

STATEMENT OF DR. DAVID O. OVERSKEI, SENIOR VICE PRESIDENT, FUSION DIVISION, GENERAL ATOMICS

Dr. OVERSKEI. I think the CIT that is presented to the government in the proposal will not ignite, but we will very shortly have the wherewithal, the additional information base from operating experiments today, as well as some further elucidation from theories, such that we will have an optimized design and be able to ensure ignition by the year 2000.

The CHAIRMAN. And what does that say about the recommendation of Dr. Hunter to withhold, I guess, brick and mortar, or should I say magnets and whatever, until we do more physics work?

Dr. OVERSKEI. I would withhold the brick and mortar, I would proceed aggressively with the requisite engineering and design in parallel with the transport initiative, which will end up with some consummate, presumably, physics model that will indicate with some surety to us within the community and to you within Congress that this machine as it is proposed will definitely achieve the goal of ignition.

The CHAIRMAN. What you are saying is there is no delay, no three- or four-year delay?

Dr. OVERSKEI. I would not delay for four years at all. I think we will have the requisite information within 18 months to two years. I think we will be ready to go out flat out in two years.

The CHAIRMAN. But we should delay at least that long?

Dr. OVERSKEI. I think we should proceed with the parallel design and R&D in support of the CIT as we are doing the theory and the expansion of the experimental database.

[The prepared statement of Dr. Overskei follows:]

STATEMENT FOR THE RECORD

SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT
COMMITTEE ON ENERGY AND NATURAL RESOURCES
UNITED STATES SENATE

DR. DAVID O. OVERSKEI
SENIOR VICE PRESIDENT
FUSION DIVISION
GENERAL ATOMICS

JUNE 13, 1989

I am David O. Overskei, Senior Vice President of the Fusion Division of General Atomics, located in San Diego, California. Thank you for the opportunity to appear before you today and offer my testimony in support of the U.S. magnetic fusion energy research program.

As some of you may know, General Atomics has had a long-standing interest in and commitment to the peaceful uses of atomic energy. GA's fusion research program began in the late 1950s and presently encompasses over 260 scientific, technical, and support personnel. We are proud of being not only the largest industrial participant in the U.S. magnetic fusion research program, but also of our independent fusion research and materials development programs using our own internal funds. These expenditures have been in excess of $10M since 1980 and have led to a number of discoveries that have benefited not only the U.S., but the international fusion research effort.

Under contract to the U.S. Department of Energy we have designed, assembled and now operate the DIII-D experimental tokamak device, the second largest tokamak in the country. Our DIII-D program involves a successful collaboration on fusion research between the U.S. and Japan. Other major fusion research efforts include fusion theory, applied plasma physics, fusion technology, and materials development. In addition to the above mentioned Japanese collaboration, we have active collaborative fusion research efforts with four national laboratories, eight U.S. universities, and six European programs.

THE SCIENCE OF FUSION RESEARCH

In the late 1920s the word plasma was first used to describe the ionized gas of electric discharge tubes. Magnetized plasma effects are critical in the determination of galactic evolution, stellar emission and radiation properties, solar flare disturbances, solar wind, and magnetospheric physics and equilibrium. With the dawning of the nuclear age, scientists turned their attention to the dream of replicating the nuclear fusion process which is the tremendous energy source of the stars. Controlled fusion research for the purpose of energy production began in earnest in the early 1950s and has continued with the much appreciated support of Congress. With success will come the realization of a limitless supply of energy capable of changing the future of the U.S. and the world.

PROGRESS

Since the modest beginnings of the magnetic fusion energy program, we have developed and tested a number of fusion concepts. In the mid 1960s the Soviets announced spectacular results from their new tokamak, a concept that posed great potential as a fusion reactor. The U.S. fusion program immediately launched an ambitious tokamak confinement effort. In response to the oil embargo of 1973 the U.S. accelerated the search for alternative energy sources and increased fusion funding. With a renewed national commitment to energy independence, significantly enhanced funding, and access to state-of-the-art technology, the U.S. fusion

program quickly became the premier fusion research program in the world. However, we became victims of our own early success, envisioning energy production in as little as 20 years. Having not yet attained this goal should not detract from the major accomplishments and demonstrations that have already been realized. The innovation and dedication of our scientists and engineers is evidenced by the rapid progress that has been made in 20 years of tokamak research in the U.S. A few examples of this program and their significance are listed below:

ACHIEVEMENT	SIGNIFICANCE
Plasma ion temperature of of 300 million degrees C in TFTR	Fifty-fold increase over temperature obtained in 1969. Above the 150 million degrees C minimum for a fusion reactor.
Hot plasma density of 10^{21} particles/m^3 in Alcator C	Fifty-fold increase over densities obtained in 1969. Above the 10^{20} particle/m^3 minimum for a reactor.
8% beta (the ratio of plasma pressure to confining magnetic pressure) in DIII-D	Fifty-fold increase since 1969. Above the 5% required for future reactors.
Confinement parameters $nT\tau_E = 4 \times 10^{20}$keV s/$m^3$ in TFTR	10,000 fold increase since 1969. Now within a factor of 10 of the value required for an ignited, energy producing plasma.

The demonstration of the above mentioned parameters would not have been possible without equally impressive gains in our understanding and ability to predict complex plasma behavior and performance. A particularly powerful example is the recent demonstration of 8% beta in the DIII–D tokamak at General Atomics. The design of the machine and precise stability characteristics of the high temperature plasma were prescribed theoretically some years ago. These recent results, combined with numerous experimental results from elsewhere in the U.S. and abroad, have allowed us to verify and substantially improve the theoretical models predicting plasma stability. These models are now being used to design next generation experiments and fusion reactors of the future. In addition, the stability theories are being applied to various phenomena which occur in other environments, such as magnetospheric disturbances and the generation of solar flares and prominences.

Other examples include the development of effective methods of heating a plasma to reactor temperatures. This was a serious problem some 10 years ago. To solve it, the fusion program embarked upon an extensive theory and technology effort to design, test, and develop neutral particle beams, high power radio waves, and microwave sources. As a consequence, the heating of plasmas well above the minimum

reactor-required temperatures has been demonstrated and is well understood, verifying theoretical predictions. In addition, these technologies are now being applied to other programs such as SDI, commercial communications, and advanced radar systems. Similarly the study of fusion plasma/material interactions has contributed to the field of plasma processing, now critical to new semiconductor technology. And lastly, the fusion program has fostered the development of large superconducting magnets, a technology critical to future fusion reactors, Magnetic Resonance Imaging (MRI) in which GA is now engaged, and more powerful high energy physics experiments.

From our position today the theoretical, experimental, and technological progress is noteworthy, but it may not seem particularly spectacular. However, from the perspective of the late 1970s, which gave birth to the research efforts which have realized this progress, it is very impressive indeed. The Toroidal Fusion Test Reactor (TFTR) has already produced 10's of kilowatts of fusion power and has already attained reactor level temperatures and $nT\tau$ demonstrations in excess of expectations. These accomplishments are much more significant than the value of Q. The Alcator C has successfully attained the Lawson parameter of $n\tau \sim 10^{20} s \cdot m^{-3}$ as planned. The Doublet III and DIII-D devices have demonstrated controlled attainment of high beta plasmas through shaping and auxiliary heating as predicted. Finally, initial operation of the Advanced Toroidal Facility may have demonstrated the existence of the second stability regime, one of the fundamental motivations for the experiment. Existence of such a regime could lead to a superior fusion confinement system.

THE NEXT STEP

Where does the fusion program go from here? We must have as our first priority continued operation of the present machines and facilities thereby broadening the data base that will lead us to a successful demonstration of a burning (ignited) plasma and a viable reactor concept. The present facilities not only represent an enormous investment, but they are producing results that continue to advance the state-of-the-art. However, upgraded performance of these machines through the addition of new heating and diagnostic systems is required to verify the theoretical physics models and confirm performance predictions that will lead us confidently to the next step. This approach, which represents a fraction of the original capital costs, will allow us to realize the full potential of the present experimental facilities, maximizing the return on the significant investment of taxpayers' money.

At the same time we must look ahead to the next stage, producing an ignited or burning plasma. A burning plasma is significant because it means that the fusion reaction will have become self sustaining, producing hundreds of megawatts of fusion power without the requirement of additional heating from external sources. Unfortunately, it is not possible to operate any of our present machines at ignition or burning plasma levels. They simply were not designed to attain those conditions. Thus, we must prepare to embark upon a national program to build a new ignition physics facility, with full participation by all the experts in the U.S. fusion community. The heart of this new facility would be the Compact Ignition Tokamak (CIT)

a device that would, for the first time ever, achieve a controlled and self-sustained fusion reaction on earth, producing several hundred megawatts of fusion power–a miniature sun in a laboratory! In support of this next step the Department of Energy has launched a transport initiative and a re-direction of effort to address the one task that has eluded us to date–the ability to predict energy and particle transport from first principles. We already have an impressive experimental data base. As a result of the transport initiative, which is based on our present experimental and theoretical capabilities, I have confidence that we will have an adequate physics model in the near future to confirm a CIT design and an operating window within which the goal of a burning plasma could be realized.

In parallel with this transport initiative we should proceed with the requisite design and technology development that will be required for the future ignition experiment. With this strong theoretical, empirical, and technology foundation, we also need the continued support of the Congress to proceed with the rapid construction of a CIT. Our national goal can and should be to demonstrate for the first time ever a controlled thermonuclear fusion burn and associated energy production by the end of the century, barely 10 years from now.

THE FUTURE

There is growing awareness that the preservation of an acceptable environment and the trend of global warming is a long-term international problem. In this context fusion energy has the promise of constituting a major element of the long-term world-wide energy supply and should be a key element of U.S. energy policy. Since the economically developed countries consume most of the presently available sources of energy, they also pose the major risk to the environment with the production of CO_2 and other toxic effluents through the burning of hydrocarbons. It is thus appropriate that the technologically developed countries proceed to jointly demonstrate the feasibility of fusion through the design and implementation of the first engineering test reactor.

This effort is presently manifested by the ITER process, in which the U.S., the USSR, the European community, and Japan have joined together to develop a conceptual design for a major engineering test reactor slated for operation about 15 years from now. It is appropriate that we continue to participate in this international process. The cost of this demonstration facility will most likely be beyond a level that any one of the participants would fund independently. For the U.S. to participate meaningfully in this process we must sustain an aggressive and productive internal fusion research effort. The construction of an ignition experiment will indeed be a major commitment by the U.S. and will again position the U.S. as a leader in fusion research. With the operation of an ignition experiment, and the design and eventual operation of an engineering test reactor, fusion research will move from a scientific to a technology-driven program. We must, therefore, now prepare to develop the technologies that will be necessary to realize the full promise of fusion as an environmentally safe energy source. This includes low activation

materials, new high heat flux and high strength alloys and composites, the development and testing of new generations of superconducting magnet and requisite heating technologies.

It is thus appropriate that the U.S. now consider ways in which industrial involvement can be significantly increased, as has already been encouraged by the National Research Council in its recent report "Pacing the U.S. Magnetic Fusion Program." I would propose that the most expedient way to do this is to assign the next phase of the U.S. fusion program participation in the ITER design process to U.S. industry with the national laboratories in a support role to industry. This will be a very effective way of transferring the necessary fusion technology from the national laboratory to the industrial system. Such an increase in industrial participation will position the U.S. to be ready to develop a fusion economy and compete on an equal footing with the other international industrial players when the engineering test reactor and subsequent demonstration reactors are actually constructed.

The magnetic fusion program represents U.S. scientific research and development at its finest. With the long standing support of Congress we have accomplished much in advancing our understanding of the world around us. We are now in position to launch a final assault on the long sought goal of ignition. With sufficient support from the Congress this goal can be achieved in the late 1990s and will constitute one of the most significant scientific and technical achievements of man.

In closing I would remind you that there is tremendous need for an environmentally sound national energy policy. I strongly support the National Research Council's view that fusion energy should be a part of this energy policy. The cost is a small premium to pay for insurance for the future. You have been elected to make policy and care for the security and well being of your constituents. However, I hope you won't forget you are also the caretakers of our future and the future of our children. Please support us in our effort to leave them with a better world. Thank you.

The CHAIRMAN. Gentlemen, let me apologize for having to leave. We simply have more irons in the fire, and the whole name of this scientific game is money. And we will deal with money in another context here with the supplemental appropriations conference, which I have to be on.

If you will stay and allow Dr. Cooper, not as an official committee meeting, but—so, with that apology, the official committee is adjourned.

And if the reporter would stay, Dr. Cooper may ask a couple of more questions. Thank you very much.

[Recess.]

Senator CONRAD. Just to indicate that we are back on the record.

Dr. FURTH. Would you like me to recapitulate?

Senator CONRAD. Yes.

Dr. FURTH. With apologies to the audience, now that we are back on the record, what I wanted to say was I wanted to complete my statement about what does the rest of the world think about us and our activities in fusion. And the statement is that we are full of surprises. We surprised them in the late 1970s by wanting to build an engineering test reactor all by ourselves, to start construction in 1982 and operation in 1990. And they were equally surprised when, in the early 1980s, we simply dumped that project and decided the United States did not need an engineering test reactor.

They were surprised again in the middle of the 1980s when the United States argued that the physics of ignition was sufficiently uncertain so that we needed an extra machine, namely the CIT. That is something that we do strongly believe and put up a strong argument for, and I think has been partly accepted. But what has not been accepted is the idea that the CIT or anything like it is a precondition for an engineering test reactor.

None of the plans of the other major players—that is, Europe, Japan and the Soviet Union—are based on the idea that they will wait with construction of an engineering test reactor until they have an ignition experiment. The arrangement is that the United States will go ahead with it; that we hope to attract from them participation and support, some of which has been offered, that they think this is valuable experience for learning how to operate their engineering test reactor in its deuterium tritium phase.

But they do not think that this information is needed in order to define what sort of coils and vacuum vessel they are going to build, any more than we now think that we need great additional information on the coils and vacuum vessel of the CIT. Those things are more fundamental and more obvious.

So they kind of tolerate us and our argument that more physics is needed and, you know, take a benevolent attitude toward us.

Now comes the next surprise. Here we have been arguing so vociferously that we need the CIT because there is all this physics that needs to be understood, and along comes Dr. Molvig and tells us but if the physics is not understood, then you should not be building a machine. And that kind of wipes us out because then there is not much room left.

But our belief is that the physics is well enough understood to go ahead, that we will learn more physics, and that we will contribute significantly to the engineering test reactors.

Well, more surprises. In the late 1970s there was a proposal made by the United States that the tokamak should be brought in competition with the mirror machine because it is more amusing to have a competition, rather than to have a single leading line. And a budget was made that helped pull back the progress of the tokamaks and accelerate the progress of the mirrors. And that surprised the rest of the world because that was not their view at all. They thought that was an odd thing to do.

And in the mid-1980s we suddenly abandoned that policy. And that surprised the world again. And it cost us about $.5 billion. And what it did was not accelerate the mirror program, but it did significantly decelerate the tokamak program and cost us the world lead.

And so I would guess that when the world hears of the proposal made today by Dr. Hunter, that we have a new plan of the same kind, where tokamaks will now be forced into a kind of artificial parallelism with inertial confinement, where tokamaks are held up and inertial confinement is accelerated, so that allegedly they will get on the same time scale, I think the world will be surprised once more, and they will say something like deja vu all over again, or something of that sort.

So, to come to a conclusion. The rest of the world runs a more steady and predictable program, which does not mean it is inflexible, it just means, you know, there is continuity. And the rest of the world tends to worry about us because they see us as being erratic. And I think we should pay attention to the rest of the world, both to learn from them that predictability is a good, productive thing, and also, as has been pointed out by Dr. Dreyfus, if we want to be treated as a sensible participant in world fusion research, then we need to be taken seriously; and then we need to behave in a way which other people will respect.

And so as to what we should do. I think we should do what was mentioned earlier, namely to hold open public hearings on fusion where the proponents are allowed to present their plans. We should assemble a truly distinguished panel of high-level, impartial scientists to review these plans. We should proceed deliberately to make a well-considered plan. And then we should stick to it.

Now, the magnetic fusion program, at the moment, has a plan, which Dr. Baldwin has briefly mentioned to you. CIT is its centerpiece. Everyone has agreed to that. Everyone also agrees that we need balance, not just CIT. We all agree that as we go along CIT can be refined. That is why we, Princeton that is, has recently created a truly national steering committee, the CIT Project Policy Board, so that we will benefit from the advice of all and fold in their views and do the right thing.

We, then, together, the U.S. fusion community will go forward to this review process. We will present our plans. We will see what people think. But I would suggest that while that is happening we do not junk what we have got, we do not dismantle our capabilities.

I agree totally with Dr. Overskei, what is going on now in R&D and design must keep going forward. And if a year from now, with the advice of a truly distinguished panel on fusion, some other direction is indicated, let us take it. But let us not do something precipitous and destructive at the moment.

I think that is all I am going to say. And thank you for listening so patiently.

Senator CONRAD. If I might ask Dr. Furth, you, on several occasion, referenced the rest of the world, indicating they were surprised by changes in strategy that we have undergone here in this country. What do you mean when you make a reference to the rest of the world?

Dr. FURTH. I mean primarily the Europeans, the Japanese and the Soviet Union. We are tied to them through a web of cooperation agreements, both bilateral and multilateral. We sit down and reason with them frequently.

Hitherto, it has been a very collaborative arrangement. But, more and more, we are being seen as the sick man in this arrangement. And I think particularly the latest developments will give greater anxiety to our partners about this partnership.

Senator CONRAD. And when you indicate that the rest of the world is surprised, what is the evidence that you would marshal that, in fact, they have been? I mean, what are the indications to you that they believe the changes that we have made were unwise?

Dr. FURTH. I would like very much to seize this opportunity to offer you a written document on this point. And I will lovingly clip out to you those signs.

As to the very latest, they have not heard yet. And presumably their reactions will be coming in.

[The information referred to follows:]

Princeton University
Plasma Physics Laboratory

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July 5, 1989

The Honorable Bennett Johnston
Appropriations Committee
Subcommittee on Energy Research and Development
SH-136 Hart Senate Office Building
Washington, DC 20510-1802

Dear Senator Johnston,

I very much appreciated the opportunity to testify to the Subcommittee on Energy Research and Development on June 14, 1989. While you were still chairing the session, I began to discuss the peculiarities of U.S. fusion program planning relative to the rest of the world program. Subsequently, Senator Conrad asked me to document my comments, and I prepared the enclosed note for attachment to my testimony.

Your cautionary remarks at the start of the session made a strong impression on me and left me convinced that we must find a way toward definitive experimental advances within our budget constraints. The present technical outlook seems to me to be sufficiently good and the collective potential of the world fusion effort is sufficiently great, so that -- with a more constructive style of program planning -- we should be able to join in a new round of major progress towards the world goal of practical fusion power.

A truly independent high-level review process could provide the starting point for the development of an effective fusion program plan for the 1990's. In the meantime, I hope that the Congress will be able to protect the resources and continuity of the U.S. fusion effort at the level requested in the President's FY'90 budget proposal.

I should like to thank you for your patient and constructive guidance over the years, and ask for your help in bringing about a well-considered future program for U.S. and world fusion research.

Sincerely,

Harold P. Furth
Director

gjc
Enclosures (2)

U.S. Participation in the International Magnetic Fusion Program*

H. P. Furth
Princeton Plasma Physics Laboratory
Princeton, New Jersey 08543

1. Introduction

Since the declassification of magnetic fusion research in 1958, there has been a strong and steady growth of international cooperation. At the present time, there are major activities in joint planning of research and exchange of data, and there is an extensive web of collaborative research projects, including long-term exchanges of personnel and equipment and an integrated design effort on the International Thermonuclear Experimental Reactor (ITER) -- a major joint enterprise that was launched by the Reagan-Gorbachev summit meeting in Geneva in 1985.

Until the late 1970's, the U.S. played a highly regarded leading role in this world collaboration. During the past decade, however, the unsteady course of U.S. planning for magnetic fusion has surprised the rest of the world research community and has aroused concern that the U.S. may prove to be an unreliable partner in future joint undertakings.

Section 2 of this note contains a brief outline of plans for the development of magnetic fusion as currently envisaged in the rest of the world. Section 3 gives some past examples of unique and transient initiatives

*This note was written in response to a request made by Senator Conrad during testimony by H. P. Furth to the Subcommittee on Energy Research and Development of the Senate Committee on Energy and Natural Resources, June 14, 1989.

in the U.S. fusion research program. Section 4 indicates the reaction of our international partners in fusion research, and Section 5 discusses the potential value to the U.S. of more predictable and steadfast participation in the world effort.

2. International Magnetic Fusion Research

The world program is pursuing magnetic fusion as an energy goal and has focussed on the tokamak concept as a realistic means for implementing an Engineering Test Reactor (ETR) and for achieving the initial commercialization of fusion power. In addition to participating with the U.S. in the joint design study for the ITER project, our principal partners in world fusion research are preparing their own national ETR projects. The Europeans have the NET, the Japanese have the FER, and the Soviets have the OTR. During the early 1980's, these projects began to be pursued seriously, with a view to implementing national (or bilateral) construction projects at the end of the 1980's. At present, the ITER schedule calls for a preliminary decision by 1991 as to whether to move towards a joint construction project. If an alternative course is followed -- for example a bilateral European-Soviet collaboration -- construction of an ETR device is likely to start in the mid-to-late 1990's.

The rest of the world has invested a considerable effort in toroidal magnetic confinement concepts other than the tokamak. This effort has gone mostly into the stellarator, with some research being done on the reversed-field pinch (RFP). There are also modest efforts in mirror-machine confinement and inertial confinement. In the rest of the world, none of these alternative concepts has been viewed as a serious competitor to the tokamak for utilization in an ETR. The programmatic role of the alternate concepts is

generally envisaged as enriching the understanding of plasma physics and providing possible longer-term flexibility for the optimization of commercial fusion power.

3. Unique U.S. Fusion Program Excursions

During the latter part of the 1970's, the U.S. program was moving towards construction of the world's first ETR device, which was first called the ETF and then became the somewhat less ambitious FED. According to the Magnetic Fusion Energy Engineering Act of 1980, the FED was to enter experimental operation by 1990 "or sooner". The U.S also participated in an international ETR design study of that period, the INTOR, but the U.S. point of view was that the implementation of an actual ETR construction project depended on the U.S. taking a strong national lead with the FED.

By the end of 1982, the U.S. decided to abandon plans for the FED and to substitute the more modest goal of a "Burning Core Experiment" (the BCX -- also called TFCX), which was restricted to the study of the plasma-physics aspects of an ETR. By 1986, the U.S. decided to abandon the TFCX in favor of the still more modest CIT, which was restricted to the study of the short-pulse plasma-physics aspects of an ETR. The fusion program leaders of the rest of the world were at first strongly opposed to the idea of inserting an extra step (the BCX or CIT) between the present generation of large tokamaks and the ETR. Subsequently, they accepted the U.S. argument that some significant remaining issues of plasma confinement physics should be clarified experimentally by a CIT project that would enter operation early in the construction phase of the ETR and provide helpful guidance for the D-T burning phase. As of 1989, the U.S. concept of the CIT mission appears to be changing again, with DOE moving towards a strategy where CIT construction is delayed

until after the key issues of plasma confinement physics have been clarified by other means.

In the area of alternate concepts, the U.S. program launched a unique approach in 1978: to slow down tokamaks and accelerate mirror machines, so as to be able to put them into near-term competition, beginning with a contest for embodiment in the ETR project. By 1986, the U.S. program had built the MFTF-B, an experimental mirror machine comparable in scale to the world's largest tokamaks -- which had meanwhile been making major advances toward the reactor plasma regime. Following completion of construction, the MFTF-B was mothballed and the U.S. mirror research program was abandoned entirely*. During the three following years, the tokamak was pursued in the U.S. as the primary approach to the ETR and the initial commercialization of fusion power. As of June 1989, DOE has proposed that the tokamak should be slowed down and inertial confinement should be accelerated, so as to be able to put them into near-ter competition for embodiment in an ETR. Such a development would set the U.S. fusion program, once more, on a unique strategy relative to the rest of the world.

4. World View of U.S. Program Planning in Fusion Research

Official foreign comments on the quality of U.S. planning in fusion research tend to be tempered by diplomatic restraint. The gap in formal documentation is partly filled by U.S.-published reports based on interviewswith foreign sources -- for example, the NRC-sponsored

*The leadership of the Soviet mirror research program is said to have requested, on first learning of the U.S. tokamak-mirror competition idea, to be exempted from the contest. An assurance of this kind seems to have been given by Soviet authorities: science-oriented mirror research continues in the USSR.

report Cooperation and Competition on the Path to Fusion Energy (National Academy Press, 1984). Some representative quotes drawn from p. 66 of this report are: "The United States is perceived as being an 'unreliable partner' -- not having a firm commitment to develop fusion, nor having a sound development plan... Furthermore, U.S. fusion policy is perceived to change much more frequently than that of either the European Community or Japan". During the five years since these words were written, the appraisal of U.S. program planning by the rest of the world has not softened -- and news of the latest U.S. initiatives is reported to have aroused a level of reaction that might finally overcome the inhibitions of diplomacy.

5. The Value of Constructive U.S. Collaboration in the World Fusion Effort

Unilateral U.S. decisions are bound to have some impact on world research policy -- but given the U.S. record of programmatic vacillation in fusion research (as well as in other fields of energy research), the most likely outcome of U.S. withdrawal from frontline magnetic fusion experiments is that the rest of the world will decide to carry on without us. An increasingly close collaboration between Europe and the Soviet Union would seem natural, and would be likely to culminate in construction of a joint ETR project of the NET-OTR type in Central Europe during the latter part of the 1990's. The Japanese national program in magnetic fusion research (which is about ten times larger that the Japanese program in "civilian" inertial confinement fusion) is in relatively sound budgetary health, and seems unlikely to undergo a U.S.-style programmatic discontinuity during the 1990's.

Since the U.S. does not have the option to "hold back" world progress in magnetic fusion -- only the option to fall behind world standards -- there is a good deal to be said for seeking to maintain our role as a credible

partner. The inflow of new ideas, technical data, research personnel, and special equipment from abroad is already providing a significant "multiplier effect" in enhancing the cost-effectiveness of our own funding in the magnetic fusion area. As the world fusion community progresses toward joint construction projects, the effective multiplier will continue to grow. To the extent to which the U.S. fusion program can convince the rest of the world of its seriousness and long-term reliability, there is even the prospect that foreign participants could be persuaded to join us in supporting major experimental initiatives, such as the CIT project, located inside the U.S.

Senator CONRAD. All right. I appreciate that very much.

Dr. Molvig, perhaps you have indicated an interest in responding to something that Dr. Furth has said.

Dr. MOLVIG. Thank you, Senator.

I did want to make two points here. The main point that got confused earlier in all our various comments about the CIT was this issue of whether the departmental recommendation constituted a delay. This term has been used several times; clearly Senator Johnston understood this to imply a delay. I want to state clearly that this is not necessarily the case.

The Department is not necessarily delaying the completion date of the project. What it is doing is insisting on certain minimum physics requirements before we have a line-item project before Congress. It is assumed that the design R&D is going to continue, and what is being proposed here need not in any way delay the completion date of the experiment, the CIT. So that is one point for the record, which has certainly been confused. And I am grateful for the opportunity to try and correct it a little bit.

The second issue is regarding whether or not this proposal that we have some minimal physics understanding to design these machines on is really the key programmatic recommendation. And I think it needs to be understood that this ITER experiment that has been discussed, that Dr. Furth has made mention to as an international collaboration, has almost exactly the same physics problem and uncertainty with regard to ignition that CIT does. And to embark on a project of that financial magnitude with physics uncertainty, I respectfully submit is folly. And we should certainly have the basis at some point.

But the Department is recommending that we stop now and say these physics problems will be solved at CIT before you go on to ITER or something beyond that.

Thank you, Senator.

Senator CONRAD. Thank you.

Dr. Furth, any last comment you would like to make before we——

Dr. FURTH. Well, there seems to be a convergence here, because I do think that what Senator Johnston may have understood was there was a proposal now to drop everything and wait four years; whereas what the Magnetic Fusion Advisory Committee said is do everything that you are doing now. Note the fact that according to the present plan, no metal will be cut, no holes will be dug until after 1992.

And so, without in any way impeding the progress of our CIT effort, we can have a physics checkpoint. And I think what Dr. Molvig just said at least comes moderately close to that point of view. So I was very relieved to hear him say that.

Senator CONRAD. See how quickly we are able to resolve things here in Washington?

Dr. MOLVIG. One final clarification. I believe the recommendation was suggested to withdraw the project, but this need not delay the completion date of the experiment was the point I very much wanted to make.

Thank you.

Senator CONRAD. Thank you very much.

Dr. Overskei, your statement?

Dr. OVERSKEI. Thank you.

Since it appears as though the individuals on the committee have probably read the testimony, I would just like to cover a few additional items, and then you can ask some questions.

I would first of all like to point out that in spite of the fact that a number of people, and accurately, recognize that the fusion program has been ongoing for very close to 40 years, the tokamak research program has really been going in this country for just barely over 20 years. And it is the tokamak program that has generated all the success, and appears to be the present foundation upon which we are proposing to build the successes of the future as well.

In the past 20 years the tokamak program has attained a performance increase of a factor of 10,000. And it is this success that has led us into the direction of a compact ignition tokamak, and has led us to the next step, which we all feel is an ignition experiment.

Now, the one thing about the CIT, as has been brought out by a number of people, is that there are some uncertainties that are associated with it. And this has been some of the motivation for the transport initiative and some of the redirection on the part of the present Department of Energy. But I have full confidence that within a relatively short time period, something on the order of one-and-a-half to two years, we will have a very accurate physics model that will explain our already substantial empirical database—or experimental database. And based on this model, we will be able to confirm a CIT design that will attain the goal of ignition in a well defined operating window.

The other thing that is worth noting is I believe that we have the technical wherewithal today to build an ignition experiment and attain ignition in a tokamak device by the year 2000. This, I would like to point out as an aside, is substantially different from that of the inertial confinement program, where substantial work needs to be performed on the technical aspects of the driver.

And I think this is a point that has not been made to date and should be recognized by the members of the committee.

The other thing I would like to point out is that our future is somewhat coupled to the role that the international players play. And as has been pointed out by Dr. Furth, the international players at this point are the Japanese, the European Community and the Soviet Union. This international program has been initiated or embodied in a process called ITER. And we think it is a significant process and something that very likely should continue.

But I would like to propose that the committee consider one other aspect—the committee and the Department of Energy—that in order for this sort of a process to be really fruitful, I think the Department of Energy should seriously consider transferring the complete next phase of ITER to the industries. It is about time that industry play a major role, which they have not done in the U.S. fusion program to date. And it is about time, if I understand the role of the National Labs correctly, it is about time that the National Labs support the industries in fusion, rather than do the

fusion work at the expense and in spite of the desire to participate that the industries have.

This would have several aspects that would be quite positive to the U.S. I have full confidence that fusion will be realized as an energy resource, but one's ability to utilize that resource and generate a fusion economy is going to be dependent upon the ability of the industries to actually participate and to build the fusion systems.

And unless we take some active role soon, we will be in a situation where we will be procuring our fusion systems of the future from the Japanese and the Europeans. And that which I am proposing would be a way in which one would alleviate this and signify to the international partners that the U.S. is serious about a major fusion program by getting industrial participation in now at an early stage.

Lastly, I would like to point out that a comment that was made earlier about why is it taking so long—I believe this was made by Senator Johnston—he was surprised as to why it was going to take so long for fusion to become a reality as far as an energy producer. The fundamental differences that we still have not given to you the scientific demonstration of ignition, and that is what we are aiming to do.

The magnetic fusion program is aiming to do that by the year 2000. That would mean that from the year 2000, we think we could produce electricity by the year 2030. So that is 30 years from scientific feasibility to producing electricity.

Now, that is not an electrical economy, which I believe the Senator was worried as to why it would take so long. If you go from the demonstration of electricity production to an economy in about 20 or 25 years, that is quite aggressive. And going from scientific proof of principle to actually demonstrating electricity in 30 years is also quite aggressive.

So I do not see that this is a significant problem. It would be comparable to the Shipping Port in the late 1950s. And it is also worth noting that the demonstration of the fission process was initiated with a significant effort, albeit for a different purpose, but the Manhattan Project indicated that it could be done, and it was then the Shipping Port in 1957, and 30 years later you do have a system.

So I do not think we are totally disparate here with regards to our ability to apply the technology in a meaningful time scale.

Lastly, I would just like to point out that we realize and very much appreciate the fact that our desires and our goals are rather long term. But the ladies and gentlemen of the Congress and the Senate are certainly the ones that have been elected by the people to take care of the present and future security of the country.

And we think that you have the vision and the leadership so that you will continue to support us and generate an energy policy which will care for the security of our posterity and take care of the future of our children. And that is all we are trying to do here. And we do hope that we continue to get your support to carry on with our goal.

Senator CONRAD. Thank you very much for your statement as well as the written testimony. Would you have any comment that you would want to make on the testimony of Dr. Furth?

Dr. OVERSKEI. I would much rather just respond to questions.

Senator CONRAD. Dr. Baldwin?

Dr. BALDWIN. I wanted to respond to what appeared to be the last remaining difference of the people on the panel, and that is whether there was any value in pulling the project—I think that was Molvig's term—and whether if you pull the project you really could fulfill the promise of achieving ignition on the same date, later down the road.

I really feel that is not a practical alternative. If the project is pulled and a new decision is made in one or two years' time, to take the optimistic scenario that Dr. Overskei suggested, given the budget time and so on, it would be about four years before you could get started again. The team which is now assembled would have been dissipated. You would have to go into a crash program to achieve that ignition in the same 10-year time frame that you are aiming at right now because you have lost four years of the 10.

We in the program, in fact, are very concerned about the budget profile for this machine. We have in place a team now. We have a slow start, but we will try to build up, as the engineering and the scientific basis warrant. This checkpoint which we have discussed is obviously the critical element of that, but it is a way to use what we have in place now as sensibly as we can.

I see no value at all for throwing that out the window with the idea that we will start again in a few years.

Thank you.

Senator CONRAD. Any other reactions that any of the witnesses might want to make?

Dr. FURTH. I would like to thank you for listening to us so patiently. And I sure hope you can help.

Senator CONRAD. We intend to be helpful. We think this is important work for the country. And we are eager to see it proceed. And as you know, there are bumps along the way. I must say what Mr. Baldwin just indicated strikes me as having some wisdom to it.

Those of us who are engaged in the budgeting process here and active participants in the appropriations process know the hard reality of stopping and starting. Sometimes when you stop you never start again. And I think we have to keep in mind the budget climate that we are operating in here, which is very difficult. It is very easy to lose momentum and difficult to regain it.

Dr. Overskei, you had an additional——

Dr. OVERSKEI. Well, I was just going to make another comment with regards to ICF. It is worth noting that there is significant progress that has been made in the development of drivers over the last several years. Nonetheless, one of the fundamental difficulties about ICF is the overall efficiency of the system. They would have to establish, a technologically-driven program, over the next several years to produce a driver. And then at that point they would have to achieve more than a factor of 100 gain in their experiment just to produce the equivalent amount of energy that would be consumed by their driver system.

The magnetic fusion program is in a somewhat different situation. The beauty and the bane of our program is that we now have the technology available to us. That is not what necessarily has to be developed, with some very relatively modest programs in comparison to the ICF. But we are pursuing the one thing, which is the beauty, it is the uncertainty of the plasma physics. It is also the bane because we do not know exactly what it is. And it makes it somewhat difficult for us to predict. But that is also why we are doing the experiment.

There are some theoretical models that say that it will be quite good when we get to the ignition physics regime. And so we do have a difference in the two programs, where we think we have most of technology ready, we need to do the experiment to address the questions. ICF, they already have some information, which is mostly classified, but they do not have the technology available.

So it seems somewhat unfortunate that you would want to penalize a program that has made so much progress, has most of the technology ready, and it would seem somewhat unwise to transfer the funds from a technologically mature program to put them into a program that is less technically mature and then say that it equalizes things. Because what it does really is retard, rather than equalize.

Dr. FURTH. That is the sort of thing I was trying to indicate that surprises the rest of the world when we do it. Overskei put it far more eloquently.

Senator CONRAD. All right. Well, I thank all of you for coming here today and testifying and participating. It is helpful to us, and we appreciate it very much.

With that, this hearing will stand adjourned.

[Whereupon, at 4 p.m., the hearing was adjourned.]

APPENDIXES

APPENDIX I

Responses to Additional Questions

Department of Energy
Washington, DC 20585

August 3, 1989

The Honorable Wendell H. Ford
Chairman
Subcommittee on Energy Research
and Development
Committee on Energy and Natural Resources
United States Senate
Washington, D.C. 20510

Dear Mr. Chairman:

On June 14, 1989, Dr. Robert O. Hunter, Jr., Director, Office of Energy Research appeared before your subcommittee to discuss magnetic fusion research, and development and demonstration programs.

Following that hearing, you submitted written questions on behalf of Senator Bill Bradley to supplement the record. Enclosed are the answers to those questions, which also have been sent to Senator Bradley.

If you have any questions, please have your staff call Frances Bryant on 586-4277. She will be happy to assist.

Sincerely,

Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosures

Department of Energy
Washington, DC 20585

August 3, 1989

The Honorable Bill Bradley
United States Senate
Washington, D.C. 20510

Dear Senator Bradley:

On June 14, 1989, Dr. Robert O. Hunter, Jr., Director, Office of Energy Research appeared before the Subcommittee on Energy Research and Development of the Committee on Energy and Natural Resources to discuss magnetic fusion research, and development and demonstration programs.

Following that hearing, you submitted written questions through Chairman Ford for our response to supplement the record. Enclosed are the answers to those questions, which also have been sent to the subcommittee staff.

If you have any questions, please have your staff call Frances Bryant on 586-4277. She will be happy to assist.

Sincerely,

Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosures

POST-HEARING QUESTIONS AND ANSWERS

RELATING TO THE

JUNE 14, 1989, HEARING

BEFORE THE

SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

COMMITTEE ON ENERGY AND NATURAL RESOURCES

UNITED STATES SENATE

WITNESS: DR. ROBERT O. HUNTER, JR.

DIRECTOR

OFFICE OF ENERGY RESEARCH

U.S. DEPARTMENT OF ENERGY

QUESTIONS FROM SENATOR BRADLEY

Magnetic Fusion Energy

Question 1: Could you please state for the record your background (including education, employment, publications, patents, etc.) in the areas under discussion at today's hearing. Specifically:

a) What is your expertise relative to the science of magnetic fusion?

b) What is your expertise relative to inertial confinement fusion (ICF)?

Answer: A copy of my curriculum vitae, describing my professional experience, is attached.

U.S. Department of Energy
Assistant Secretary for
Congressional, Intergovernmental
and Public Affairs
Office of Public Liaison
Washington, D.C. 20585
202/586-4292

BIOGRAPHY

ROBERT O. HUNTER, JR.

Robert O. Hunter, Jr., was nominated by President Reagan to be Director of the Office of Energy Research, U.S. Department of Energy (DOE) on May 25, 1988. Mr. Hunter was confirmed by the United States Senate on August 11, and sworn into office on August 18, 1988.

In this position, Mr. Hunter serves as technical adviser to the Secretary on the Department's energy research and development programs and is responsible for the oversight of several single and multipurpose national laboratories, and energy education and training activities. In addition, he manages DOE's programs for basic energy research, health and environmental research, high energy and nuclear physics, and magnetic fusion.

Prior to joining DOE, Mr. Hunter was the President of the Western Research Corporation in San Diego, California, since 1978. He served as a member of the White House Science Council from 1982 to 1985, and between 1973 and 1978 he was a staff scientist at Maxwell Laboratories. He has worked primarily in the areas of laser physics, optical physics, and electrical engineering.

A native of Riverside, California, Mr. Hunter received his B.S. degree in physics from Stanford University in 1967, and his Ph.D. degree in physics from the University of California, Irvine, in 1981. He served in the United States Air Force from 1967 to 1972.

Mr. Hunter is married to the former Becky Vasiliki and they have three children.

August 1988

RESUME

ROBERT OLIN HUNTER, JR.

Birthdate and Place:	1 November 1946 Riverside, California
Address:	PO Box 9381 (6285 Via Campo Verde) Rancho Santa Fe, CA 92067
Home Telephones:	(619) 454-9446/(619) 756-0714
Current Position:	Director, Office of Energy Research U.S. Department of Energy
Business Telephone:	(202) 586-5430

Family Information:

Wife:	Becky Vasiliki Hunter
Children:	Christine Vasiliki, born 7 September 1976 Alexandra Nichole, born 14 November 1980 Nicholas Spero, born 6 May 1984

Education:

B.S., Physics, Stanford University (1967)

Ph.D., Plasma Physics, University of California, Irvine (1981)

ROBERT OLIN HUNTER, JR.

August 1988 - Present: Director, Office of Energy Research
(Assistant Secretary of Energy rank)
U.S. Department of Energy
Washington, D.C.

Manages DOE's program for basic research. These duties entail signature authority over $2.1B of scientific and engineering research ranging from the most fundamental (high energy physics) to highly applied research (materials sciences such as x-ray lithography). Specific areas comprise basic energy sciences, health and environmental research, high energy and nuclear physics and magnetic fusion. This comprises the activities associated with siting, construction, and eventual operation of the Superconducting Super Collider (SSC) project, as well as major scientific facilities for use of industry, universities, and national laboratories in various fields. Examples of such facilities are nuclear reactors for research, large accelerators, and x-ray synchrotron sources.

Also serves as the technical advisor to the Secretary for the full range of the Department's energy research and development programs.

Responsible for the oversight for the Department of several single and multiple program national laboratories, including Oak Ridge National Laboratory, Argonne National Laboratory, Brookhaven National Laboratory, Fermi National Accelerator Laboratory, and Stanford Linear Accelerator Center. Major aspects include the institutional plans, oversight and accountability of these laboratories as related to their missions and performance.

June 1987 - January 1988, June 1988 - August 1988: Consultant, U.S. Department of Energy

October 1978 - April 1988: President and Chairman of the Board
Western Research Corporation
San Diego, California

Founded Western Research Corporation (WRC) in order to pursue development of large scale lasers for energy and defense applications. The company grew from 1 to 80 people and is now largely devoted to defense research and development. For energy application, in 1979 invented a method called optical pulse compression of KrF lasers that is the basis for providing the large energies necessary for inertial confinement fusion (laser fusion) at low cost. Designed and built the world's largest excimer laser for Los Alamos National Laboratory.

For the defense application, applied Raman scattering for beam combination and beam cleanup. These techniques were experimentally demonstrated in obtaining a near diffraction limited output at the 1 joule level in 1982 and the 200 joule level in 1985 and are the basis for scaling lasers to the very large energies required for the Strategic Defense Initiative (SDI). Performed both theoretical analyses and experimental work in the course of these activities. In 1985 the world's largest XeCl laser was activated at WRC for SDI research. The fastest adaptive optic was developed at WRC for the adaptive optics involved in the Strategic Defense Initiative. In April 1988, Western Research Corporation was sold to Thermo Electron Corporation.

ROBERT OLIN HUNTER, JR.

February 1982 - February 1985: Member, White House Science Council

As a member of the White House Science Council, participated in the studies and policy formulation relating to basic research, national industrial policy, defense policy and other areas. Was particularly involved in defense aspects as a member of the panel assessing requirements for nuclear weapons material production and a member of the panel evaluating advanced technologies for new weapon systems. Participated in studying the long-term vitality of our national laboratories, and the future of engineering education. Involved in both pre and post SDI discussions of role of defense and pace of development.

September 1974 - June 1981: Student, University of California, Irvine
Obtained Ph.D.

While working in industry, obtained a Ph.D in Theoretical Plasma Physics. These dealt with certain optical scattering phenomena in the sun's interior: a major problem in experimental physics has been the discrepancy between observed and predicted fluxes of neutrinos emitted by the sun. Developed a model for the effect of shield cloud fluctuations on on the photo-ionization cross section of high Z elements as utilized to estimate the size of the effect and identify other scattering phenomena of interest to the intermediate coupling regions characteristic of the solar interior. My advisor was Professor Norman Rostoker. Received a Fannie and John Hertz Foundation Fellowship.

April 1973 - September 1978: Staff Scientist, Maxwell Laboratories
San Diego, California

Served as the Head of the Laser Program and Leader of Team B, a group of four aerospace companies developing large visible and ultraviolet lasers for the Defense Advanced Research Projects Agency (DARPA). Work performed in my group included the largest rare gas halide (350 joules at 2500 A with 8% efficiency from KrF in 1976) and vacuum ultraviolet lasers (8 joules at 1700 A with 3% efficiency from Xe_2^* in 1974) built, as well as the first measurements of the Group VI lasers. Also engaged in work on ion beam generation, carbon dioxide laser systems, and fast switching. A principal participant in the discovery (obtained laser with coworkers at 1260 A in argon) and development of vacuum ultraviolet rare gas excimer lasers, the ultraviolet rare gas halide lasers, and the mercury halide visible lasers. Inventor of electron beam controlled switching. Also involved in the discovery and kinetic analysis of several other lasers.

ROBERT OLIN HUNTER, JR.

March 1970 - October 1972: Laser Physicist, Air Force Weapons Laboratory
Kirtland Air Force Base, New Mexico

Performed duties in three main areas of laser technology, invention, and development. Also responsible for system analysis of electric lasers, and power systems analysis and programmatic direction. Led a group that designed and constructed high pressure electron beam sustained CO_2 and CO lasers. This included first U.S. operation of a 10 atmosphere CO_2 laser and a 4 atmosphere, room temperature CO laser (1972). Received U.S.A.F. Scientific Achievement Award for this work. Invented and designed with Los Alamos National Laboratory personnel the multi-aperture electron beam sustained geometry that later became the basis for both DoD and DOE large scale gas lasers. Performed system assessment for electric lasers to go on board the Airborne Laser Laboratory. Directed work in high power airborne power supplies. Served as Capt.

March 1968 - March 1970: Nuclear Research Officer, Air Force Aero
Propulsion Laboratory, Wright Patterson
Air Force Base, Ohio

Project officer for power generation involving Magnetohydrodynamic (MHD) generators for airborne applications, including direction of contractors and other government agencies. Performed long range analysis of methods of generation of large amount of power and initiated superconducting rotating machinery and other efforts. Served as Lt.

November 1967 - March 1968: Officer Trainee, United States Air Force
Officer Training School, Lackland Air Force
Base, Texas

Commissioned a Lieutnant in the U.S.A.F.

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QUESTIONS FROM SENATOR BRADLEY

Question 2: What is the recommendation of the DOE's Magnetic Fusion Advisory Committee with respect to the near-term fusion budget and pace of funding? Did the MFAC adopt or modify the recommendations presented earlier by MFAC Panel 22? Did the MFAC recommend a cut in fusion funding?

Answer: To the best of my knowledge, the Magnetic Fusion Advisory Committee (MFAC), by letter of June 21, 1989, recommended that the potential and needs of the MFE and ICF programs be reviewed by a high-level technical committee prior to the making of any changes in the MFE budget requests, but has made no specific recommendations on the near-term fusion budget and pace of funding.

The MFAC cannot modify the recommendations of one of its panels. MFAC accepts or rejects its panel reports, which are reports to MFAC. Then MFAC, in open session, develops its findings and recommendations to DOE. It is this latter document, which also transmits the panel report, that constitutes advice to the Department. MFAC's letters of March 9, 1989, and June 7, 1989, with the reports from Panel 22, are attached. A subsequent related letter from MFAC dated June 21, 1989, is also attached.

I do not believe that MFAC has ever recommended a cut in fusion funding.

UNIVERSITY OF WASHINGTON
SEATTLE, WASHINGTON 98195

College of Engineering
Department of Nuclear Engineering

March 9, 1989

Dr. Robert O. Hunter, Jr.
Director, Office of Energy Research
U.S. Department of Energy
1000 Independence Avenue
Washington, DC 20585

Dear Dr. Hunter:

The Magnetic Fusion Advisory Committee (MFAC) met in Austin, TX on March 7-8, 1989, to consider the work of Panel 22 on (1) confinement and transport and (2) the physics capabilities of existing machines, as charged in your letter of December 6, 1988; and (3) an assessment of the probability of ignition of CIT as presently proposed, as charged in your letter of January 3, 1989.

The MFAC established Panel 22 to study and report to it on these issues on March 7-8, giving a progress report on issues (1) and (2) and presenting a final report on (3). In this letter, we first summarize the progress on Panel 22 studies on issues (1) and (2). Second, we transmit our findings relative to the ignition of CIT.

At its Austin meeting, the Chairman of Panel 22 described to MFAC a series of hearings held by the Panel on items (1) and (2) of your charge. Item (2) was not discussed in detail at our meeting. The final report dealing with these topics will be presented to MFAC at its June meeting.

The long-standing issue of confinement in tokamaks has assumed increasing importance as the fusion program projects toward larger, more expensive devices such as CIT and ITER. Fortunately, a concerted effort in this area is made possible by the status of performance on present devices, new diagnostics, and continued theoretical developments.

MFAC reiterates the support given by Panel 22 for the work of the Transport Task Force (TTF) and recognizes the enthusiasm with which the entire US community is addressing the confinement and transport issue. There has been a qualitative change and buildup of momentum in the fusion community for this task. Carrying out this task requires prompt approval for the FY1989 reprogramming actions to maintain this momentum. However, MFAC expresses concern that some of the funds have come from elements of the program previously identified by MFAC as being both essential for fusion and subcritical in funding. This is of special concern for FY1990 and beyond.

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Findings on the CIT Ignition Question

MFAC accepts the Panel 22 CIT Report (enclosed herewith) and generally agrees with its assessments and recommendations.

The Panel 22 CIT Report accurately reflects the uncertainty involved in extrapolating to ignition. It was this uncertainty which gave rise to the two-phase project approach summarized in the report. The first phase provides full coil capability, but only partial powers for the magnets and auxiliary heating, and is intended to sharpen the power requirements for the second phase. Additional auxiliary heating power during Phase 1 would be beneficial toward this end.

In spite of the uncertainty in predicting ignition, the Panel endorses the CIT concept as the proper next step for the program; and, as the general parameter range for the machine is known, it endorses continuation of the necessary design and R&D for the machine. MFAC likewise continues its support for the construction of CIT as expressed on a number of earlier occasions. This will require additional funds in future years.

The transport initiative will reduce some of the uncertainties regarding ignition. Proceeding at the same time with the construction of the CIT with its basic capability will position us to exploit fully the results of the transport initiative in the experimental phases of CIT.

I look forward to discussing these finds with you at your earliest convenience.

Sincerely yours,

ORIGINAL SIGNED BY
F. L. RIBE

Fred L. Ribe
Chairman
Magnetic Fusion Advisory Committee

FLR:ll

Enclosure: Report of MFAC Panel 22: "Assessment and Recommendations Regarding the CIT."

UNIVERSITY OF WASHINGTON
SEATTLE, WASHINGTON 98195

College of Engineering
Department of Nuclear Engineering

June 7, 1989

Dr. Robert O. Hunter, Jr.
Director, Office of Energy Research
U. S. Department of Energy
1000 Independence Avenue
Washington, DC 20585

Dear Dr. Hunter:

The Magnetic Fusion Advisory Committee (MFAC) met in San Diego on June 6-7 to consider the Panel 22 Report on Confinement and Transport, Machine Physics and CIT Ignition, as charged in your letters of December 3, 1988 and February 3, 1989.

In the San Diego meeting MFAC also heard a summary of the recent NAS/NRC study on the magnetic fusion program. This report recommends that fusion play an important part in a US energy plan for the next century, and that a timely start for the CIT is an important next step.

Panel 22 worked under a very broad charge in a short period, producing an excellent review of the status of the transport issue. We note that the analysis of the transport picture closely parallels that of the Transport Task Force (TTF). MFAC reiterates the support of the Panel 22 for efforts expended in the TTF. MFAC accepts the report enclosed herewith, but provides below our own assessment of certain issues addressed in the report.

1. While transport is an important issue and is properly being given high priority, it would be a great mistake to focus unduly on this issue at the expense of other program elements important for fusion development or to use it as a sole criterion for evaluating program activities.

2. In attempting to identify resources for the transport initiative, the Panel on several occasions identifies and questions continuation of facilities that it sees as not

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contributing to transport. This is in spite of considerable program investments in facilities that are close to completion, and which have capabilities of doing valuable fusion physics. Examples of this include the D-T option in TFTR, CPRF/ZTH, the FRC program, and to a lesser extent MTX. These programmatic decisions must be reviewed in the context of the full fusion program.

3. Fusion progress, like almost any scientific development effort, will proceed best when both empirical and theory driven means are used in conjunction. The Panel 22 report underemphasizes contributions that can be made by empirical science.

4. The Panel report calls for specific reorganizations of MFECC Computer Center activities. These recommendations reflect the changes that are occurring in large scale computing, associated with questions dealing with the efficient use of existing and newly acquired resources. Several studies, both inside and outside fusion, are examining these questions. Although MFAC expects that some reorganization will prove beneficial, it recommends not acting prematurely until this issue has been more thoroughly examined and until changes can be made in a well-coordinated way throughout the program.

5. In dealing with the question of tritium in TFTR, the NRC report arrived at the same position as that of MFAC Panel 17. Specifically, assuming the CIT is to be built, D-T operation in TFTR would provide valuable experience of tritium handling in an operating tokamak. The Panel 22 report implicitly discounts this value by dismissing tritium operation as not important for transport studies.

The D-T period in TFTR has been delayed by about three years by the transport initiative. During this time, TFTR will be making important contributions to the transport activity. In light of the concerns raised by the Panel, MFAC recommends a review of the costs of the TFTR D-T option. However, MFAC attaches high priority to maintaining this capability.

6. In questioning the value of the descoped ZTH facility, the Panel focusses on its 1.7-MA capability as a single figure of merit, rather than addressing the physics contribution that the facility would make. MFAC reaffirms the value of the RFP research program and notes the substantial resources already committed to the CPRF/ZTH construction. We support the review suggested by Panel 22 and recommend that it focus on the effectiveness of the facility in producing important physics results at both the lower and higher operating currents. We also note that when considering the roles of RFX and ZTH, the existence of comparable but complementary facilities is most valuable in developing the underlying science.

7. We strongly reaffirm our comments relating to CIT contained in the MFAC letter to you of March 9:

> "In spite of the uncertainty in predicting ignition, the Panel endorses the CIT concept as the proper next step for the program; and, as the general parameter range for the machine is known, it endorses continuation of the necessary design and R&D for the machine. MFAC likewise continues its support for the construction of CIT as expressed on a number of earlier occasions. This will require additional funds in future years.
>
> "The transport initiative will reduce some of the uncertainties regarding ignition. Proceeding at the same time with the construction of the CIT with its basic capability will position us to exploit fully the results of the transport initiative in the experimental phases of CIT."

In the subsequent meeting, Secretary Watkins expressed his support for an ignition experiment within ten years and the need for a physics checkpoint on the transport issue. In response to the sentiments he expressed, we believe that progress on the remaining transport questions will be adequate within the next few years to provide a basis for ignition; that the CIT design, R&D, and prototyping should proceed in parallel with the transport work; and that the check point should release construction funds for the project, rather than initiate them.

Because of the interest in the fusion program expressed by the Secretary, along with the potential sweeping impact of the Panel 22 Report and the foregoing recommendations on the program, I request that I discuss these findings with both you and the Secretary.

Sincerely yours,

Fred L. Ribe

Fred L. Ribe
Chairman
Magnetic Fusion Advisory Committee

FLR:shk

Enclosure: Report of MFAC Panel 22: "Confinement and Machine Physics"

UNIVERSITY OF WASHINGTON
SEATTLE, WASHINGTON 98195

College of Engineering
Department of Nuclear Engineering

June 21, 1989

Honorable James D. Watkins
Secretary, U.S. Department of Energy
James Forrestal Building
1000 Independence Avenue, SW
Washington, DC 20585

Dear Mr. Secretary:

Since the June 6-7 meeting of the Magnetic Fusion Advisory Committee (MFAC), the Department has made public the outline of a proposed initiative to establish a 10-year competition between Magnetic Fusion Energy (MFE) and Inertial Confinement Fusion (ICF), based on the Compact Ignition Tokamak (CIT) and the Laboratory Microfusion Facility (LMF) devices. Believing as it does in the importance of fusion in the nation's energy future, and in particular in the urgency of the ignition step, the undersigned unanimous membership of MFAC feels it essential that the potential and the needs of the MFE and ICF programs be reviewed by a high-level technical committee prior to any changes in MFE budget requests.

The fate of the CIT project is part of our concerns. In order to achieve ignition at minimum cost, the project was laid out with two phases, only the second of which was regarded as having high probability for ignition. In a letter to Representative Lloyd dated July 11, 1988, the Acting ER Director, Dr. Decker, described the two-phase strategy and the DOE approval of it. We take exception to the CIT being equated to only its first phase; doing so conceals the sound logic and value of the CIT project. By all accounts, including that of MFAC Panel 22, the CIT facility as represented by its full capability has a high probability of achieving its ignition goal. Given the importance of ignition in fusion's development, it would damage the MFE program to have a CIT delayed or cancelled out of concern for the ignition risk of Phase I.

If assurance of early ignition is the prime consideration, a better approach in pursuit of a timely ignition experiment would be to develop a plan for proceeding directly to the full-power configuration of Phase II. The plan should be developed with the participation of the fusion community, the Congress, and potential foreign participants. In his oral testimony to the June 14 Senate Subcommittee on Energy Research and Development, Dr. Hunter referred to the Department's policy to "push both (MFE and ICF) to achievement of ignition . . . in new experimental devices within about 10 years". In our view, for the CIT this can be done most effectively by continuing its preparation in parallel with the transport initiative, as expressed in our finding letter of June 7 to Dr. Hunter regarding Panel 22.

Regarding the proposed competition itself, MFAC has on many occasions expressed its belief that an assessment of fusion's potential as an energy source must be multi-faceted, including not only questions of physics feasibility, but also those of nuclear technology, materials, economics, safety, and environmental impact. Scientific progress is needed on a number of high-leverage issues in order to reduce the technological complexity of fusion systems and to speed the development of an attractive reactor end product. Examples of key physics issues, in addition to transport, are efficient current drive, increased beta

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(energy density), and auxiliary heating methods. These issues are now being addressed not only in the mainline tokamak but also through work on selected alternate magnetic fusion concepts, allowing both a complementary view of the underlying physics and an examination of other potential avenues to a reactor. Within available resources, the present MFE program addresses the physics and technical issues in a balanced way, in preparation for an assessment of magnetic fusion in the first decade of the next century. The effect of the proposed MFE budget cut would be to greatly weaken the ability to make an informed and accurate assessment.

The MFE/ICF competition and program restructuring proposed by the Department would limit the comparison to the performance of two devices, the CIT and the LMF, each of which is designed to address a narrow physics issue, ignition in the CIT and significant energy multiplication in the LMF. Although the CIT would be closer to its reactor goal than the LMF to its reactor goal, neither device would be prototypical of a reactor to follow, and neither device would be intended to, or be capable of, addressing the many questions that would need to be answered in assessing its line of approach to fusion energy. The competition, as posed, would not serve its intended purpose and is unlikely to assure a well defined path to commercial fusion energy.

In the attempt to create a competitive situation, the effect of the initiative would be to slow one program and accelerate the other. The $50M in funds proposed to be removed from MFE would considerably disrupt the program. They are, in part, just those necessary to resolve the transport issue and prepare for the ignition experiment; and they would, in addition, halt much of the needed multifaceted research discussed above. Out of concern for the long-term health of fusion development, we strongly urge that recommendations for major changes in the MFE program plan and for reprogramming of funds, including the magnitudes of funds to be involved, be delayed until their technical merits and impact on the affected programs have been thoroughly reviewed.

Sincerely yours,

Fred L. Ribe, Chair
Melvin Gottlieb
Tihiro Ohkawa
Charles C. Baker
Robert Kribel
Richard E. Siemon
David E. Baldwin, V. Chair
James E. Leiss
Peter Staudhammer
Everett E. Bloom
Dale M. Meade
Harold Weitzner
Wilhelm B. Gauster
D. Bruce Montgomery

QUESTIONS FROM SENATOR BRADLEY

Question 3: What is the recommendation of the National Academy of Science's National Research Council with respect to the nearterm fusion budget and pace of funding? Did the NRC recommend a cut in program funding?

Answer: A copy of the executive summary from the National Academy of Sciences' National Research Council Report, "Pacing the U.S. Magnetic Fusion Program," is attached. The summary contains the Committee's recommendations.

Pacing the U.S. Magnetic Fusion Program

A Report Prepared by the
Committee on Magnetic Fusion
in Energy Policy
Energy Engineering Board
Commission on Engineering and Technical Systems
National Research Council

NATIONAL ACADEMY PRESS
Washington, D.C. 1989

EXECUTIVE SUMMARY

This study addresses the priority and pace of the nation's magnetic fusion research and development program in the context of long-term national energy policy. In particular, the committee interpreted its task as follows: To review the implications of long-term national energy policy for current research and development in magnetic fusion; to identify factors that should enter the further development of such policy to reduce risks associated with the future electricity supply system; to propose criteria applicable to research and develop in electric generation in reaching long-term energy policy goals; to apply these criteria to magnetic fusion and alternative electric generation technologies in order to develop recommendations on the priority and pace of the magnetic fusion program; and to present its results in a final report.

The most important goals of the U.S. Department of Energy's current Magnetic Fusion Energy Program Plan are to demonstrate the scientific and engineering feasibility of fusion. Demonstrating engineering feasibility will require the design, construction, and operation of an engineering test reactor, which the plan envisions financing through a combination of domestic and international funding. The committee believes that current domestic program funding levels are inadequate to meet even the near-term objectives of the plan.

The committee considered four programmatic alternatives:

- Asserting U.S. scientific and engineering leadership by pursuing a broad-based, relatively independent U.S. program that would minimize U.S. participation in international collaborative efforts. Developing a solely domestic engineering test reactor would require funding levels substantially greater than current levels;

- Maintaining present program objectives, which the committee believes will require increases in funding above current levels;

- Maintaining current program funding levels, which will require either reducing present program breadth or deferring both the construction of a U.S. facility for conducting ignition experiments and U.S. participation in the construction of an international engineering test reactor; or

- Reducing current program funding levels, which will require narrowing the scope of the present domestic program and withdrawing from international collaborative efforts.

THE LONG-TERM ENERGY FUTURE AND ENERGY SUPPLY CONTEXT

Fusion power plants would generate baseload electricity. Consequently, they will most likely have to compete with coal-fired and nuclear fission plants employing advanced technology that can be expected to be commercially available by 2050. Other baseload technologies, such as solar photovoltaic systems including storage, might also be commercially available by that time. In principle, coal-fired, fission, and other nonfusion power-generation alternatives could supply global electricity needs in that time frame. For example, the committee finds (Chapter 3) that by year 2010 the use of coal, the most abundant fossil fuel, is projected to increase by 50 percent in the United States, and to double in market economies. However, the future of each of these technologies is uncertain. For example, coal combustion is known to contribute significantly to atmospheric carbon dioxide accumulations, further generation of electricity using nuclear fission must overcome the loss of confidence that has developed over the last two decades, and new fission technologies have not yet been reduced to commercial practice, though there is little doubt of their future commercial viability (Chapters 3 and 4).

The committee finds (Chapter 3) that today's principal end uses of energy--transportation; heating, cooling, and other residential and commercial applications; lighting; heat and power for manufacturing applications; and industrial feedstocks--will continue in the future. The proportion of total energy requirements served by electricity will continue to grow, and electricity will account for an increasingly larger share of primary energy consumption. The mix of U.S. energy uses dictates the mix of U.S. energy supplies, and changes will be slow because of a vast existing infrastructure. In the United States and other industrial nations, the current excess capacity for generating electricity is likely to disappear during the 1990s. By reducing consumption, conservation and energy efficiency measures can extend conventional energy supplies and lengthen the horizon for developing and commercializing alternate sources of energy. However, the committee concludes that these measures are unlikely to eliminate the need for new energy options during the next century.

The committee further finds and concludes (Chapter 4) that the future mix of primary energy supplies will depend on both energy and nonenergy considerations, including 1) economics, 2) environmental health and safety aspects of energy supplies, and 3) unforeseen political developments. The use of widely available fuels may be restricted. For example, as discussed in Chapter 3, coal is now the focal point of global controversies over acid precipitation and global warming. A critical priority for decision makers is to understand what responses to make to acid precipitation and to the possibility of global warming.

The committee concludes (Chapter 4) that a prudent long-term energy strategy for the United States requires that alternative electric energy supply technologies be researched, developed and demonstrated. A diversified array of alternatives is needed as insurance against the vulnerability of existing alternatives, such as coal and nuclear fission.

Thus, the committee concludes that the United States should adopt an energy-insurance strategy, developing an array of diverse technologies as hedges against serious shortfalls in coal-derived and nuclear fission-derived power. As a part of this study, the committee surveyed broadly the prospects for a number of technologies, but within study constraints was not able to assess their relative merits as a basis for allocating federal R&D funds. Thus the committee recognizes a need--not fulfilled by this study--to conduct a comprehensive comparative evaluation of demand management and electrical supply alternatives, leading to the formulation and implemention of a balanced national energy R&D program. Pending such an evaluation, the committee believes that magnetic fusion merits continuing support as a potentially important technological alternative to resolve energy-supply uncertainties such as those noted above.

THE MAGNETIC FUSION PROGRAM IN CONTEXT

The goals of the U.S. magnetic fusion program appear reasonable to the committee. The committee finds (Chapter 2) that as an energy program, the primary objective of the Department of Energy's Magnetic Fusion Energy program is to establish the scientific and technical base for magnetic fusion by about 2005-2010 to support subsequent commercial reactor design and demonstration. This timing is coordinated with cooperative U.S. efforts toward an international demonstration of engineering feasibility.

The committee further finds (Chapter 2) that the United States is no longer the dominant leader in international research on magnetic fusion. The United States lost its leadership position in magnetic fusion energy to the European Community largely because of a 50 percent decrease in funding (measured in constant dollars) since 1977. As a consequence, the U.S. program has also narrowed. Although "energy breakeven" in the next few years is a widely shared expectation, fusion ignition and burn remain to be demonstrated.

The next major frontier in magnetic fusion development is the achievement of a self-sustaining fusion plasma; namely, fusion, ignition, and burn. The next critical-path experiment proposed by the Department of Energy is the Compact Ignition Tokamak. If successful, the Compact Ignition Tokamak will have demonstrated fusion's scientific feasibility. After Compact Ignition Tokamak, the Department of Energy strategy is to participate in an international program to establish the engineering feasibility of fusion.

The committee concludes (Chapter 5) that if stated U.S. fusion program objectives are to be achieved, funding levels higher than current levels will be required. Annual funding would need to increase by about 20 percent in the near term, then by about another 25 percent during the mid- to late-1990s. Such levels would be required to enable the United States to proceed now with a fusion ignition experiment and to participate later as an equal partner in the construction of an international experimental facility intended to demonstrate engineering feasibility. These steps could also provide the United States with an opportunity to develop a U.S. industrial cadre skilled in fusion R&D, thus preparing industry for future participation in fusion development.

The committee considered (Chapter 5) but rejected three fusion program alternatives. One such alternative would be to pursue a broad-based, relatively independent U.S. program aimed at asserting U.S. scientific and engineering leadership. That program would cost appreciably more than the current program, requiring annual budgets about twice current levels to embark on the construction phase of a domestic engineering test reactor in the mid-1990s. The committee also rejected the alternative of limiting funding to current levels. That alternative would require narrowing the breadth of present program activity and removing one or more major planned program elements, thus decreasing the likelihood that magnetic fusion energy will be a product of U.S. technological development. Finally, the committee considered and rejected the alternative of reducing funding, which would eliminate further U.S. collaboration as an equal partner in the international fusion effort.

The United States, the European Community, Japan, and the Soviet Union have separate programs to demonstrate the scientific feasibility of magnetic fusion. In person-years per year, the current U.S. and Japanese fusion programs are comparable in effort, while the European Community's program has significantly greater activity. The size of the Soviet Union's fusion program is difficult to estimate, but appears to be comparable to that of the United States. Largely because of the high cost of fusion research and development, international collaboration and cooperation are increasing; the committee endorses this trend and believes that the U.S. fusion program should be based on a strong international collaborative effort (Chapter 5).

Currently, under the existing international collaborative effort, there is considerable industrial participation in the Japanese fusion program, where industry plays a major role in designing and building

fusion systems and subsystems. This attribute is largely absent from the U.S. program. If this lack of parallelism continues, U.S. companies will be at a competitive disadvantage as the fusion research and development program is increasingly internationalized and as fusion and its related technologies move toward commercialization.

COMMITTEE RECOMMENDATIONS

Based upon the considerations and study conclusions summarized on the preceding pages and in the body of this report, the committee has arrived at the following recommendations:

- The committee recommends that the federal government develop a broad, balanced array of technological alternatives as an insurance strategy for meeting U.S. and global long-term energy needs. Decisions about the government's role, which alternatives to support, and funding levels should be based on the results of a comprehensive assessment. That assessment should include the full range of energy, environmental, public health and safety, security, economic, institutional, and other attributes of each technological alternative, including magnetic fusion (see Chapter 4). In the interim, the committee offers the three recommendations below as to the priority, pace, and direction of the U.S. magnetic fusion program.

- The committee recommends that the United States enter into partnership arrangements for the next major steps in an international fusion program, as the most cost-beneficial U.S. approach to fusion over the next decade (see Chapter 5).

- The committee recommends an increase over current fusion program funding of about 20 percent, held steady for the next five-years, followed in the mid- to late-1990s by an additional increment of about 25 percent. These increments are expected to permit construction and operation of the Compact Ignition Tokamak, resolution by the early 1990s of the central scientific feasibility question, and participation in the construction of an international engineering test reactor beginning in the mid- to late-1990s (see Chapter 5).

- The committee recommends that the Department of Energy develop a revised program plan providing for greater participation by U.S. companies, to ensure that these companies are not placed at a disadvantage relative to their foreign competitors. Such participation could, for example, follow the Japanese practice of assigning industry responsibility for both the design and construction of major systems and subsystems (see Chapter 5).

QUESTIONS FROM SENATOR BRADLEY

Question 4: Did either the MFAC or NRC endorse or recommend the proposed technical "competition" between magnetic fusion and ICF?

Answer: The concept of a competition between magnetic fusion and ICF was outside the scope of the National Research Council's study entitled "Pacing the U.S. Magnetic Fusion Program." As a result, they did not carry out a review of the concept. Similarly, the MFAC has not been charged with such a review, and hence, has not provided the Department with recommendations on this concept at this time.

Neither the MFAC nor the NRC has endorsed or recommended a competition between magnetic and inertial fusion. Neither body examined inertial fusion in the studies referred to, or considered such a competition. Hence, any such recommendation would have been inappropriate.

QUESTIONS FROM SENATOR BRADLEY

Question 5: Has this fusion "competition" been proposed, reviewed, or endorsed by an impartial panel of fusion experts within the Department? Has the proposal been reviewed by the Energy Research Advisory Board?

Has it been reviewed by an independent panel of scientists under the auspices of the National Academy of Sciences or some similar impartial organization? Is any such review planned?

Answer: The competition between magnetic and inertial fusion was established as the Department of Energy's Policy for Fusion Energy in September 1978, as delivered in testimony to the Senate by John M. Deutch, then Director of Energy Research. A copy of the Policy is attached (attachment). The Department has never superseded that statement with a conceptually different overall Policy for fusion energy, although many further program strategies for magnetic fusion have been produced. The 1978 policy was the direct result of the recommendations of the Ad Hoc Experts Review Group. The Group, an independent body of fusion experts, reviewed the fusion programs for several months in early 1978. Their work remains the only independent review ever conducted of the two programs together. The fusion "competition" proposed has not yet been reviewed or endorsed by an impartial panel of fusion experts either within or outside the Department. The NAS did review the inertial fusion program in 1986 and found it had the essential structure and means to resolve significant technical issues if funded at level funding for five years; a second NAS review of the military and civilian applications of

Answer 5
continued: inertial fusion is underway. The Secretary has directed that the draft of the new policy receive a thorough independent review and assessment by an impartial panel of fusion and energy policy experts, and the Department has begun the process of establishing such a review.

DOE/ER-0018

The Department of Energy Policy for Fusion Energy

September 1978

U.S. Department of Energy
Directorate of Energy Research

This policy statement has been prepared under the auspices of the Fusion Review Committee of the Department of Energy's Research and Development Coordination Council

FUSION REVIEW COMMITTEE

Chairman: Dr. John M. Deutch, Director of Energy Research

Members: Mr. Robert D. Thorne, Assistant Secretary for Energy Technology

Dr. Duane C. Sewell, Assistant Secretary for Defense Programs

The Department of Energy Policy for Fusion Energy

John M. Deutch
Director of Energy Research

The Department of Energy's applied research program in controlled thermonuclear fusion has recently been the object of public attention because of the announcement of encouraging research results. At the same time, many people working in the program have felt concern about it because of new review processes and Federal budget deliberations. Both these sources of interest make it appropriate to describe the Department's policy for fusion energy research and development, with broad emphasis on objectives and priorities.

Because the program comprises two substantially different approaches - magnetic confinement and inertial confinement of fusion reactions - we must deal with several categories of policy matters: general objectives and strategy; policies specific to each of the two programs; and finally policies governing program review.

Context of the Program

The bulk of the Department of Energy's budget must be devoted to the solution of immediate and mid-term energy supply problems (including environmental control), primarily to the introduction of liquid fuel substitutes,

rather than to a more distant, long-range solution. We could hardly expect it to be otherwise.

Possible long-range solutions - that is, solutions which rely on inexhaustible sources of energy - all demand technologies which do not entirely exist today or are evidently too costly. So, we cannot rely heavily on such possibilities to help us out of energy difficulties during the next 20 to 30 years. When we speak of "inexhaustibles," then, we are talking of what we hope will be the next century's primary energy sources. While research and development programs involving inexhaustibles must compete for funds with projects of more immediate concern, it would be a mistake to infer that the Department does not view the fusion program as one of great significance.

Before the middle of the next century -- sometime between 2020 and 2050, by current estimates -- diminishing reserves of oil, natural gas, and fissile fuels will force us to place increasing reliance on inexhaustibles. But the inexhaustibles all require many years of development before they can make the substantial contributions.

There are three such inexhaustible sources: solar energy, fission breeder reactors, and fusion. Solar energy technologies do exist now, but most will require long development periods before they can generate large amounts of power economically. Fission breeder technology still requires years of development and demonstration, and it still faces the political and social problems of proliferation and long-term waste disposal.

Fusion energy is furthest of the three from practical economic utility, but its potential rewards are great. Successful commercialization of fusion could provide an energy resource whose ultimate fuel (deuterium extracted from water) is cheap and essentially unlimited, and whose by-products would pose much reduced environmental problems compared to coal and fission power. Fusion power stations would pose no increased risks to the community (beyond those of an ordinary fossil fuel plant), and would face no geographical limitations. But there is no lead time to spare: if fusion energy is to be available when it is needed -- thousands of Megawatts economically produced about 2030 -- the research and development program, rigorously directed toward the goal of commercial utility, must be undertaken in earnest now.

In fact, the high priority which the Department places on fusion research has recently been demonstrated tangibly. We expect the total budget for fusion to remain essentially at its present level - a total for magnetic and inertial confinement of about five hundred million dollars per year -- through fiscal year 1980. But the President has declared that the Federal budget will decline, and the Department of Energy will contribute a cut in spending. That the fusion program has been exempted from even a straight-percentage share of this cut is a strong affirmation of support. In the magnetic confinement program, the technology base is actually growing, because a smaller fraction of the budget is applied to the construction of large facilities.

This support has been strengthened through a 6-month process of formal review under the Fusion Review Committee, which was established by the Department's Research and Development Coordination Council. That review found the program to be technically sound and to possess promise of achieving economical fusion power eventually.

Decreases in funding from the current level could seriously endanger that promise, and leave us with an expensive research program but very little chance of developing practical fusion energy sources.

At the same time, increases in the budget could increase our confidence in the program's success but would not be likely (under realistic budgetary constraints) to reduce materially the time between critical choices among technical alternatives. The reasons for these limits shall be discussed later. It should now be clear that our confidence in the program's ultimate success -- its achievement of engineering and economic feasibility - is very high. But we expect that success will take many years work, more than 30 years at best. Management of the program in the interim must guarantee that it remains balanced and directed toward this product, an economic energy source.

The recent fusion review process employed two elements of import for the program's direction. First, the R&D Coordination Council includes a standing Fusion Review Committee composed of the Assistant Secretary for Energy Technology, the Assistant Secretary for Defense Programs and, as chairman, the Director of Energy Research. This Committee assures continuing attention to the fusion program by high-level DOE management. Second, the fusion review included a valuable contribution by the Ad Hoc Experts Group, chaired by Dr. John S. Foster. The formation of such an ad hoc group of outside experts, assembled to

examine both magnetic and inertial fusion programs, is a clear precedent which indicates the Department of Energy's intention to benefit from expert technical evaluation from the scientific community at large during its policy formation process.

As with all other programs, the present budget represents a compromise with fiscal realities. It is important to bear in mind the very substantial research and development costs that will be incurred as the fusion program passes through the stages of technical development, from demonstration of scientific feasibility to commercialization.

Objectives

Before outlining the phases of the program, it is necessary to be specific about its objectives.

> The goal of fusion research in the Department of Energy is to develop the highest potential for employment of fusion energy.

The highest potential cannot be developed without an extensive technical base, both scientific and engineering. When choices are made, we must be confident that they are based on a firm understanding of all significant technical alternatives. This

differs from a crash project, in which one strives to produce something that will work as soon as possible, subject only to a few minimal criteria of performance. The first device that would work at all might be far from fusion's highest potential, and the research that led to its construction would be narrowly focused.

Thus, our philosophy in designing and managing this R&D program is to provide demonstration of that highest potential as soon as possible in an economically acceptable way that fully preserves and develops the technical base. This cannot be accomplished in a sudden plunge ahead, giving all or most resources to the leading prospect at some particular time. We must assure that fusion technology develops toward an economical and practical engineering system in a balanced progression and provides as wide a range of choices as possible throughout the progression.

Though most people in the program should look toward fusion power - that is, a pure fusion reactor - as its goal, we must consider other applications as well. The phrase fusion energy is meant to include fusion power, and also various hybrid fusion concepts: devices in which, for example, fusion reactions are used to breed fission fuels, or to produce other useful fuels, such as hydrogen.

Design of the Program

In the past, attention within the program has been centered on the demonstration of "scientific feasibility," which is generally accepted as attainment of net energy gain from fusion reactions. It has been felt, with justification, that until such demonstration has been made fusion energy will seem a distant doubtful prospect. "Scientific feasibility" is necessary as the first unequivocal step in demonstrating the feasibility of controlled fusion reactions to be harnessed in a practical, economic technology.

Our confidence in making that demonstration in about 5 years is high, based on the program's steady progress over the past few years, including such gratifying results as the recent achievement of high ion temperatures in the PLT tokamak.

But we must take care not to become myopic. Demonstration of scientific feasibility is an intermediate objective, a necessary but not sufficient prerequisite for achieving the goal. As the first major objective, scientific feasibility does have the highest single priority within the program, but this priority is not granted at the expense of other objectives. Projects which aim beyond that demonstration will be supported, and in some cases expanded.

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Following the demonstration of scientific feasibility, the program will move from applied research into a development phase. Engineering Test Facilities (ETF) will be constructed for the most promising prospects in both magnetic and inertial confinement. These facilities will be integrated systems producing net energy gain using fusion plasma techniques developed in the previous generation of experimental devices. The Engineering Test Facilities will also establish the technological requirements of each of the major components of a prototype reactor.

The next phase of the program, demonstration, will involve the operation of an Engineering Prototype Reactor (EPR), which first combines the elements tested in the superior Engineering Test Facility into a pilot plant in which the unknowns of reactor design can be tested and resolved. The EPR will approach for the first time complete energy gain, where the energy produced exceeds all energy consumed in keeping the entire plant running. Finally, demonstration will be completed with the construction of one or more commercial demonstration reactors, in which net power gain in excess if 100 Megawatts per plant is produced with an economic efficiency that will make them attractive to

industrial investors. Full commercialization of fusion energy will have been accomplished when about one tenth of a quad per year, which is about the equivalent of three 1,000 Megawatt power plants, is produced; we call this the Initial Operating Capability.

The Government will remain the primary source of funding through the construction and operation of the Engineering Prototype Reactor. One need not stress that the Government will welcome investments by and cooperative ventures with industry and utilities as the program moves through development to demonstration.

We confidently expect that scientific feasibility will be demonstrated within the magnetic confinement program in the early to mid-80s. This will be accomplished by the Tokamak Fusion Test Reactor (TFTR), presently under construction at Princeton.

Although one cannot yet be quite as confident in predicting particular dates for success in inertial confinement experiments, it is possible that by 1986 scientific feasibility will have been demonstrated by one or more of

three systems: the Nova laser at Livermore, the Antares laser at Los Alamos, and the EBFA electron beam at Sandia Laboratories. I shall not discuss here the objectives or schedule of the concurrent project to develop military applications of inertial fusion.

After scientific feasibility of its general approach has been demonstrated, each program will decide which of its competing alternatives will be the basis of its Engineering Test Facility. The two decisions will necessarily involve somewhat different logic.

In magnetic confinement, we expect that tokamaks, such as Princeton's TFTR and General Atomic's Doublet III, will have demonstrated the feasibility of operation of a tokamak ETF. The question will then be: do alternative magnetic confinement schemes, particularly mirror machines, offer significant advantages over tokamaks? All such alternative devices are, so far, well behind tokamaks in achieving major objectives, and tokamaks may prove to be the best devices for fusion energy reactors. But the fact that they are ahead at this stage of research is not conclusive, and our program design must not force us to make decisions too soon. If one or

more of the alternative schemes produces highly promising results in the interim, it is imperative to delay the decision date for the magnetic ETF in order to make the most reasonable choice - the one which will develop the highest potential for commercial fusion energy.

The situation in inertial confinement research is, in a sense, precisely the converse. The leading candidate for demonstration of scientific feasibility - the Nova glass laser facility at Livermore Laboratory - is not a contender for the inertial ETF, because glass lasers cannot attain the efficiency or the repetition rate necessary for fusion reactors. The decision on what sort of ETF to build here will involve a process called driver evaluation. This is, assuming that the feasibility of inertial confinement - fusion of small pellets driven by a large energy source - has been demonstrated, it will then be necessary to choose the best large energy source to drive the pellets to reaction. Projects are underway to investigate various gas lasers, electron beams, and light and heavy ion beams as drivers. There is no guarantee that any one of them can be made to work as a practical fusion driver; nor can we be sure that

when inertial scientific feasibility is demonstrated, the best candidate drivers will have demonstrated their practicability. Again, we must be prepared to suspend judgement to guarantee that we have selected the system which will satisfy commercial requirements.

The current plan calls for final driver evaluation to take place after 1986, and for an ETF decision to be made the following year, if the state of research permits.

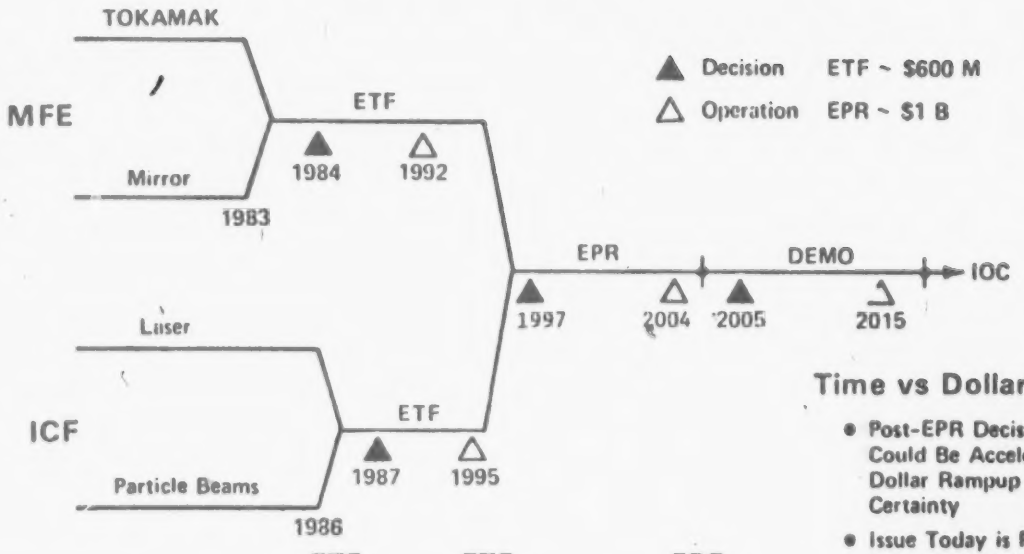
We expect about 8 years to elapse in each program between the ETF decision and initial operation of the facilities. Thus reasonable dates for operation of the two ETFs are 1992-5 (for magnetic) and 1995-8 (for inertial), assuming funding follows the current projection. That projection keeps the entire program's operating budget roughly constant in 1979 dollars and estimates each ETF at about $600 million. Between now and the time both ETFs are operational in the late 90s, we will have spent about $11 billion. In order to be in a position to make ETF decisions in the mid-to late-80s, and thus to have ETFs operating in the mid-90s, attention to engineering development must be intensified now.

Experience with the ETFs will teach us what sort of system -- magnetic or inertial confinement -- we want to make the basis of the Engineering Power Reactor, and how to build it. That construction will take another 8 years and will cost in the neighborhood of a billion dollars. The EPR should be fully operational by 2005, with the decision on the design and funding of the commercial demonstration reactor in progress. At that point the entire fusion program will have expended about $18 billion. The Initial Operating Capability would accordingly be estimated as 2020-2030, a time frame consistent with current predictions of the necessity for large contributions from alternative energy sources.

The program schedule and its milestones are shown schematically on the accompanying figure. This current projection of the program's timetable should not be taken to be immutable, for several reasons. First, we are sensitive to the fact that both scientific and engineering uncertainties can influence the pace and character of the program. Still, over two decades of progress in magnetic confinement research, and about half as much in inertial, justify reasonable confidence in the establishment of these planning milestones. This confidence is exists not only within the Department of

Fusion Development

(Reference Budget Case)



Time vs Dollar Tradeoff

- Post-EPR Decision Program Could Be Accelerated By Dollar Rampup with Greater Certainty
- Issue Today is Program Through ETF Period Up To EPR Decision Point

Key Milestones	ETF Decision	ETF Operation	EPR Operation
Cumulative Costs To Key Milestones	$4 B	$11 B	$18 B

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Energy management, but in the scientific community, as the report of the Ad Hoc Experts Group shows. Second, we must recognize that this schedule can be modified by the rate at which resources are committed to the program. And third, we must be constantly prepared to accommodate substantial innovation: the program's schedule must be sufficiently flexible to include the introduction and exploration of entirely new conceptual approaches.

All these considerations point to the necessity of assessing the status and prospects of the programs as realistically as possible, and of focusing on the near term of the projected schedule as a means of maintaining flexibility and preventing premature commitment to a single approach. The important near-term decision points concern the ETF scope and the choices among ETF design prospects. At least until that point is reached, we must continue to proceed on a broad front, which means intensive investigation of many conceptual approaches. We recognize as valuable assets the program's momentum and the expertise of the corps of researchers providing it; they are assets acquired at great cost, and we shall insure their continuity both in the DOE laboratory system and in outside technical institutions.

As was explained in discussing the program's objective, proceeding on a broad front also means that, through ETF operation and beyond, we must support a vigorous research base in plasma physics and engineering. Maintaining plasma physics research involves the familiar problem of protecting the technical base in a program which involves large and expensive hardware. Both the magnetic and inertial programs are continuing to build expensive machines, useful devices which are capable of absorbing money and talent out of supporting research at precipitous rates. We must not permit ourselves to become so preoccupied with the achievement of milestones on large devices that we have no resources devoted to understanding what is really happening, and thus to maintaining progress when something goes wrong. If we occasionally find that the research base or balance cannot be guaranteed without postponing for a year or even 2 years the operation date of a large facility, we must be prepared to face that postponement.

Increasing the engineering research base commands an equally high priority. Engineering and materials test projects, coupled with system designs and analyses by engineering experts, will be expanded in both programs. And as the

programs mature from applied research to engineering development, we must encourage a shift in machine design and construction from DOE laboratories and universities to industry. Analysis and development of hybrid reactor concepts must be considered integral to an effective engineering research program.

One note of caution must be added concerning the significance of projected schedules and milestones. Fusion advocates and supporters of particular fusion schemes and approaches understandably are tempted to "oversell" the program by publicly emphasizing dramatic milestones. In a program that is both so costly and so important to our Nation, it is necessary to encourage a responsible and sober appraisal of anticipated and achieved technical progress. We must be careful also to distinguish between real scientific objectives, which involve proof of our increased understanding of systems, and the necessary technological milestones, which involve tests of high performance of large experimental devices. The maturation of scientific and engineering bases requires that we proceed at a measured pace through the complex development process.

In summary, the near-term objectives of DOE fusion policy emphasize:

- o Demonstration of scientific feasibility
- o Establishment of sound engineering development base supporting the ETF decision
- o Maintenance of a strong scientific base
- o Encouragement of research in alternative concepts, and scrutiny of their positions relative to the established major projects.

Magnetic Fusion

As has been said, the magnetic confinement program has been largely dominated, both technically and financially, by tokamak research. As a result of this predominance, tokamaks have recently provided a series of gratifying and promising successes. In increasing support of alternative concepts, it is not our object, as some might say, to penalize the leader because it is the leader.

The crux of it is that tokamaks, for all their promise, appear to possess disadvantages as reactors. Their complex geometry may lead to difficulties of operation, cooling

and maintenance. It will be difficult to operate them in steady-state, to control impurities efficiently and to obtain direct conversion of energy in charged reaction products. Although none of these problems eliminates the possibility of an economically competitive tokamak reactor, the coincidence of them suggests that, as we try to find methods of overcoming them by improving tokamak design, we vigorously seek new schemes which, by their conceptual nature, avoid the problems altogether.

Mirror machines present one such conceptually attractive alternative concept, though at the moment they remain scientifically behind tokamaks in demonstrated plasma confinement capacity. The question we must answer is whether this deficit is solely a result of the greater resources and attention which have been devoted to tokamaks. It may also be possible to combine the advantageous features of toroidal and mirror systems, and several concepts which attempt to do this are being studied. Many other schemes are equally far behind the mirrors themselves, yet some of these have conceptual advantages that would make them, if they worked out, highly attractive.

Yet if we continue to support all these schemes at basic-research level, we are guaranteeing that none of them will be actually competitive when the magnetic ETF decision is made. So it is necessary that, over the next 2 years or so, we choose those alternative concepts which show the most promise of providing significant advantages over tokamaks and devote available resources to them. This winnowing process is already underway. Because of our confidence in tokamaks, we are not simply asking of each project, "Will this work?" but also, "If it does, will it be much better -- from engineering and economic points of view -- than a tokamak?"

We do not expect to be able to make and operate an economically satisfactory power plant unless significant progress is made on a number of critical technical issues, among which are:

1. Detailed understanding of particle and energy confinement in tokamaks, and of scaling laws for such confinement; limitations on plasma and magnetic field density.
2. Limitations on the pulse length of tokamaks operating under burn conditions;
3. Impurity control in tokamaks;

4. First wall conditions and lifetimes in both tokamak and mirror reactors;
5. Viability of the tandem mirror concept, including maximum plasma density and energy gain achievable;
6. Detailed understanding of particle and energy confinement in tandem and field-reversed mirrors, including radial transport electron heat conduction in tandem mirrors and magnetic islands in field-reversed geometrics.
7. Limiting powers and efficiencies of auxiliary heating techniques for both tokamaks and mirrors;
8. Safe, practical techniques of energy removal and utility at high efficiency from any magnetic confinement systems (including, in the applicable systems, direct conversion to electricity); and of course,
9. Viability of other alternate magnetic confinement systems.

Inertial Confinement

Inertial confinement research is less mature than magnetic confinement research. We are concerned that the technology base in inertial confinement is not yet as broad, in terms of number and diversity of skillful professionals employed, nor as deep, in terms of basic physics and engineering

issues well in hand. The difference in time and resources expended on the problems accounts for most of this disparity.

It is time to give increased emphasis to broadening and deepening the program's technology base. Specifically, we hope to establish a second major target research and design capability similar to the highly expert group at Lawrence Livermore Laboratory. We also wish to emphasize the importance of basic physics experiments - studies of the detailed mechanisms of energy coupling and implosion. These experiments can for the most part be carried out on existing devices, such as the Argus and Shiva lasers at Livermore and the Helios laser at Los Alamos, and the lasers at KMS Fusion and the University of Rochester.

Regardless of the construction schedules of larger devices, we must maximize the experimental utility of existing devices to guarantee our solid understanding of the dominant physics issues of inertial confinement. Some of these issues we take to be:

1. Detailed understanding of absorption mechanisms on pellet targets, including plasma instabilities, preheat effects, stimulated backscatter processes and transport inhibition mechanisms;

2. Detailed understanding of the physics of ablative-pusher compressions, and of ignition processes;
3. Detailed understanding of fluid-plasma instability mechanisms in targets, and thus of requirements for uniformity of illumination;
4. Optimum pulse length and actual power regime needed to satisfy the strictures of all the preceding processes;
5. Detailed understanding of effects of these processes on wavelength scaling for laser drivers, and on particular choice of particle for other drivers, as these effects prescribe limits for driver performance;
6. Understanding of the internal physics limiting the scaling and design parameters in each of the contemplated driver systems: CO_2 lasers; other gas lasers; electron beams; light and heavy ion beams.
7. Safe, practical techniques of energy removal and utility at high efficiency from any inertial confinement system.

Increased emphasis must be placed within the inertial program upon research on complete systems - drivers and targets together. For it is the complete system which must produce practical and

efficient energy. We cannot say that certain drivers are inherently more practical or even efficient apart from the energy-generating systems of which they must be parts; certainly the same is true of target design and fabrication.

Finally, we must consider the matter of classification of inertial confinement research. Some of this research is presently classified because of the potential military applications of this technology. At present, the near-term objective of inertial fusion work is to assist our nuclear weapons programs; however, with the demonstration of inertial confinement's scientific feasibility, we would expect to see a split to an energy technology program, and a military applications technology. In the interim, we are seeking the most reasonable way to define classification priorities, a way that permits maximum participation in the research while guaranteeing the security of the sensitive military information.

Review and Coordination

The formation of the Department's Fusion Review Committee and the efforts of the Ad Hoc Experts Group demonstrate the Department of Energy's commitment to continuing review of the research and development program in controlled fusion.

The review process has proved to be of great benefit to the program in a number of ways. Communication and cooperation between magnetic and inertial confinement research has been improved. Perhaps most important, the program has taken a major step toward the establishment of the technical and managerial stature it deserves with other members of the scientific community, administrators and legislators.

We therefore intend to continue the Fusion Review Committee, and we shall periodically obtain advice from external experts of stature in the scientific and engineering communities. These further Ad Hoc Experts Groups may be constituted as parts of the soon-to-be established Energy Research Advisory Board. The Groups' primary responsibility will be to assist in determining the major scientific and engineering objectives of the programs and of the projects themselves, and to review regularly progress toward those objectives. The forum of the Ad Hoc Experts Group will provide an opportunity for open review and critique of progress and plans, and will assure that individual objectives remain congruent and pertinent.

We expect that through such an ad hoc review process, increased cooperation between the magnetic and inertial confinement research programs will be fostered. A good interchange of plasma physics research data already exists. We intend to extend this to pooling of engineering research data and cost information for reactor studies, and eventually to the establishment of common measures of performance for proposed reactor designs and reactor subsystems.

The national laboratories will necessarily remain the centers around which the Department organizes a large part of its fusion research programs. But, as has been emphasized, integration of more scientists and engineers from the industrial and academic communities into the program, as a means of strengthening the research base, is an important and specifically designated goal. We hope also to expand our current efforts toward international cooperation in fusion research, to include more (and larger) jointly funded projects with the Japanese, Europeans, and perhaps the Soviets. Negotiations for such increased cooperative ventures are already underway. These projects would not be mere gestures of political harmony, but could be of significant value to the progress of the programs.

We are convinced that the programs are making real progress, and that scientific feasibility will be demonstrated in about 5 years. We intend to continue development from that point to the concrete establishment of practical, economic fusion energy sources. The Department's confidence is reflected in its firm support of the fusion budget. There is much to be done, but we can reasonably expect that fusion will become an important world energy source in the early part of the next century.

DOE/ER-0008

Final Report of the Ad Hoc Experts Group on Fusion

June 1978

U.S. Department of Energy
Directorate of Energy Research

This report has been prepared for, and at the request of the Fusion Review Committee of the Department of Energy R&D Coordination Council.

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Dr. Donald M. Kerr, Acting Assistant Secretary for Defense Programs

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Dr. Burton Richter, Stanford Linear Accelerator Center

Dr. John W. Simpson, Consultant

Executive Secretary: Dr. Robert A. Summers, Department of Energy

With the assistance of the Fusion Review Staff:

Dr. Alexander J. Glass

Dr. Michael Roberts

Department of Energy
Washington, D.C. 20545

JUN 7 1978

MEMORANDUM FOR John M. Deutch
Director of Energy Research

FROM: Ad Hoc Experts Group on Fusion

SUBJECT: Report of Findings and Recommendations

On February 27, 1978, you requested the individuals identified below to conduct a brief review of the magnetic and inertial confinement fusion programs and report their findings and recommendations at an early date. We have met on two occasions--the first time for three days of briefings by headquarters and laboratory people, and the second time for two days to discuss our views and to generate this report. On each occasion, we were grateful for the opportunity to meet with you to clarify to us your interests and needs.

In addition, our sincere appreciation goes to the presenters from DOE headquarters and its laboratories for their efforts to improve our understanding of the fusion programs. In formulating our observations and recommendations, we were stimulated and benefited from discussions with Bob Summers, Alex Glass and Mike Roberts. Without their hard work we would not have pulled together this report.

In presenting this report it is important to recall that while we have been exposed to a wealth of information, it was not our desire nor your intention that we cover all of the important aspects of the programs which warrant careful review and assessment. Rather, as agreed, we concentrated our attention on just four areas: the objectives of the programs, the strategies being pursued, the program balance, and some aspects of management. In each of these areas, we found the need to advise you on our findings and suggest recommendations.

Ad Hoc Experts Group

Solomon J. Buchsbaum
John M. Dawson
John S. Foster, Jr., Chairman
Edward T. Gerry
Michael M. May
Thomas M. O'Neil
Burton Richter
John W. Simpson

Executive Secretary: R. A. Summers, DOE

Attachment:

Letter dated February 27, 1978, from
John M. Deutch to John S. Foster, Jr., Chairman

7 June 1978

I. Objectives

There are now in view only a few essentially inexhaustible energy sources which can in principle contribute to the solution of the long range world energy problem. These include fusion, fission (with some type of breeder), and solar power, each of which must be pursued to provide the nation with an adequate set of energy options. A program based on fossil hydrocarbon fuels, though necessary for the near-term, provides only a limited solution for a combination of four reasons: (1) resource limits (oil in the short-term, and coal in the medium term), (2) national security (vulnerability of supply to interruptions not under our control), (3) geopolitical (nonuniform distribution of resources and the increasing need of the developing countries for energy), and (4) environmental (CO_2 and consequent climatic effects, particulates, trace chemicals, etc.).

Since fusion energy is one of a severely limited number of possible alternatives, the first objective of the program must be to determine the highest potential of fusion as a practical source of energy. To this end, a number of different conceptual approaches to producing fusion reactions must be evaluated, a number of difficult engineering problems must be investigated (neutron damage and tritium handling, for example), the best partners in a marriage of scientific and engineering promise must be selected, and the cost of a practical system must be determined. The final phase of the first stage of the program in which the government is still the primary sponsor will be construction of one or more engineering test facilities followed by an engineering prototype reactor(s). Only then can a decision to develop fusion energy for the economy be made with reasonable confidence.

The commercialization stage will include the development and operation of commercial demonstration reactors and the deployment of the technology in the economy. The minimum time from the beginning of the commercialization stage until fusion can contribute significantly to the nation's energy supply is about two decades. The decision whether or not to use fusion for commercial power will be made in the context of the promise of other long term options.

For the potential of fusion to be effectively determined over the coming years, an overall guiding policy and consistent set of objectives are needed. The Group believes that there is an urgent need to answer the questions concerning feasibility and that the momentum of the program developed over the past few years is a major asset and should be maintained. Obviously, the time required to

determine feasibility is influenced by the level of funding made available to the program. The appropriate strategy and associated programs are considered in the next section.

II. Strategy

Assuming that the objective is to determine the highest potential of fusion as a commercial energy source, it is important to give careful attention to the selection of the proper strategy. In our judgment the proper strategy should optimize the chances of determining the highest potential for a commercial fusion energy source at the earliest practical date, in the face of several current alternative physics concepts and many downstream technological and economic uncertainties.

The strategy which we recommend for the next several years is to pursue fusion on a broad front; broad in the sense of vigorous support to several different conceptual physics approaches, and broad in the sense of an intensive physics/engineering analysis, tradeoffs and experiments to identify and resolve problems which could stand in the way of a practical fusion reactor. In our judgment it is too risky at this stage to select and provide specific emphasis on one of the several major concepts. Also, it is too risky at this time to concentrate just on the physics experiments and delay to a later time considerations of downstream engineering problems. Rather, with diligence and good luck we would hope to find one or more acceptable combinations of physics and engineering concepts which would seem to provide viable paths to fusion energy.

The recommendations made in the preceding paragraphs are of necessity general in nature and do not address the issue as to whether total fusion budgets should be increased or decreased in the competition with other energy program funding. Translation of these recommendations into detailed programs and budgets is a complex task which should receive the highest priority. This task should be undertaken by the program management in concert with the technical community.

The magnetic confinement program currently rests in the main on experiments with Tokamaks, with the mirror program carried as a back-up. This strategy, as discussed below, places undue emphasis on a single approach, which constitutes an unnecessarily high risk. The commitment at this time to the construction of a next generation Tokamak project would further increase the risk. The strategy

which we recommend requires in essence increased efforts in areas other than Tokamaks in order to allow a realistic choice to be made from among several hopefully viable alternatives. A reason for this choice of strategy is that, while Tokamaks are currently the most advanced scientifically, they seem on the surface to be the most complex of the possible alternative approaches to fusion from the standpoint of engineering into an energy producer.

The inertial confinement program is constructing the facilities necessary to implode pellets and investigate driver-target interactions. While the present program is quite appropriate to an early achievement of scientific feasibility, it lacks adequate emphasis on the development of drivers which will provide the system energy efficiency necessary for a practical energy source.

The strategy we recommend requires changes in balance and emphasis in both the magnetic and inertial confinement programs. Currently, most of the experiments are directed toward the demonstration of scientific feasibility. However, even as this is being accomplished, there must be a major increase in emphasis on engineering problems to provide the basis for proceeding to engineering development and the subsequent choice of an engineering prototype reactor. Ultimately, it is the promise of a particular approach to perform in a routine fashion in, for example, a power grid, which should determine the choice of a particular fusion option. We emphasize again the necessity to pursue in parallel with scientific experiments, engineering feasibility studies of the most promising alternatives. Engineering designs of each concept should be carried to sufficient detail to determine the nature of the engineering requirements and the magnitude of the problems involved.

III. The Magnetic Fusion Energy (MFE) Program

The magnetic fusion program is a mature program which has set itself a well-defined goal under which it has been operating, i.e., to develop and demonstrate fusion central station electric power. There are two main approaches, the closed magnetic confinement approach, e.g., Tokamak (accounting for about 60% of MFE budget), and the open magnetic confinement approach, e.g., mirrors (20%). In addition, money is spent on technology supporting present experiments (6%), and advanced concepts and technology for future experiments and power reactors (14%).

Tokamaks

At present, the U.S. magnetic fusion effort is dominated by Tokamak research. This is largely justified by the degree of success these machines have had in the past few years, as represented by the product of density and confinement time $n\tau \gtrsim 3\times10^{13}$ cm^{-3} sec. and ion and electron temperatures $T_i \gtrsim T_e \gtrsim 2.3$ KeV, - values which are within modest factors of those required for reactors. It appears that this approach has a high probability of achieving breakeven or with the Tokamak Fusion Test Reactor (TFTR) even net instantaneous fusion energy production (net with respect to the neutral beam power required to maintain the plasma); with very good luck ignition might even be achieved. Since a primary near-term goal of the fusion program is to achieve a reacting plasma, this effort should be pushed aggressively.

Despite their successes, some major questions remain for Tokamaks concerning the following issues:

1. The detailed physics of plasma and energy confinement times; theoretical explanation of the observed scaling laws.

2. The critical ratio of plasma to magnetic field pressure $\beta = (4\pi P/B^2)$ (P is plasma pressure and B is magnetic field strength) at which the plasma can operate.

3. Their behavior under long pulse conditions (can they be made steady state?).

4. Impurity control.

5. Lifetime of the first wall.

6. Practicality of Tokamak reactors. Are there significant ways to simplify them, reduce their size, improve their economics and engineering desirability?

Items (1), (2) and (4) are being attacked under present experimental programs. Addressing of points (3) and (5) requires the development of more advanced facilities. Point (6) is one of considerable concern to the utility industry and its supporting manufacturers and emphasizes the need for early engineering evaluation of reactor concepts.

Tokamaks may provide the earliest means to obtain a reacting or burning plasma on which physics studies can be made, and which could be used for materials and engineering tests. If present devices, as well as those under construction, i.e., TFTR (1981), Poloidal Divertor Experiment (PDX) (1979), Alcator C (1979) are pushed hard, a reacting plasma may be obtained in the early 1980's. To exploit these devices fully some upgrading of the machines may be required such as more beam power, higher energy beams, added radio frequency heating, or increased magnetic fields. While substantial burning may be obtained in devices presently under construction (TFTR and Joint European Torus, [JET]), they are not designed for the long-term burns suitable for engineering studies. Nevertheless, special Tokamaks may provide the earliest means for studying burning plasmas and long duration discharges at thermonuclear-equivalent conditions, and performing materials and engineering tests for reactor components.

The present profusion of Tokamaks which exists is probably the result of a "bandwagon" effect and represents an overextension of the program in this direction. Given fiscal constraints, the best possible use should be made of the existing facilities, including consideration of closing some facilities so as to extract the maximum information from others. If additional monies are available, these might be more productively spent on existing facilities than for initiating new devices.

Mirrors

Mirrors have achieved impressive plasma parameters, as represented by $T_i \simeq 20$ KeV, $\beta \sim 1$. Mirrors have always been envisioned as energy multipliers. Their uncertainty lies in whether the marginal energy multiplication (Q) predicted for classical mirrors (Q 1) can be improved upon significantly. There are at least two ingenious ideas to improve Q, namely:

(1) The tandem mirror.

(2) The field reversed mirror.

Critical questions for these devices are the stability of reversed field mirrors, which plasma losses will dominate in these devices when the more rapid mirror losses are reduced, the seriousness of electron heat conduction for tandem mirrors, what electron temperatures will be achieved in these devices, and how high a Q can be

achieved. The Tandem Mirror Experiment (TMX) (1979) and Mirror Fusion Test Facility (MFTF) (1983) facilities will provide tests for these ideas and may provide answers to these critical questions.

If either or both of these ideas succeeds, then mirrors may make attractive energy sources of modest size, capacity and expense relative to Tokamaks. It is also likely that they will be simpler to construct. If the fusion hybrid is considered, the relative simplicity of open magnetic confinement or linear machines may be particularly attractive.

To determine the feasibility of the mirror approach as an alternative reactor option, the TMX and the MFTF deserve an aggressive effort.

Alternative Concepts

There are many alternative magnetic concepts, in fact about a dozen or more (field reversed pinches, long solenoids, Linus, multipoles, etc.) It is clear that an attempt to support any large fraction of them will simply spread the money so thinly that none will have a significant chance.

However, there are two areas in which significant breakthroughs might be made which could change the whole picture. One is the area of "advanced" fusion fuels which could have considerable influence on environmental impact, engineering simplicity of the reactor, plant lifetime, etc. The second area involves devices which, with D-T fuels, could make a practical reactor considerably simpler to engineer, construct and maintain.

Using these considerations, only the most promising alternative concepts should be chosen and supported at a sufficient level (several million dollars/year) and for sufficient time (4 or 5 years) to test the concepts. They should then either be upgraded or dropped.

Plasma Physics

Plasma physics and our understanding of it is still a pacing item in fusion research (witness the relatively recent rise of Tokamaks as contenders for reactors and the recent and unexpected favorable dependence of Tokamak confinement time on density, $\tau \sim n$, found in Alcator). This effort could lead to improved machines with better confinement, concomitant reductions in size, and improvements in economy. It should receive substantial support which is shielded

against the funding needs of large hardware programs. Some support should be aimed at basic plasma physics to develop the understanding and the tools needed in applications to the fusion program. Perhaps such additional support should be provided by the Office of Basic Energy Sciences.

IV. The Inertial Confinement Fusion (ICF) Program

At present, the ICF program is less mature than the MFE program. It has fewer teams working in it, a less broad background of scientific effort, a smaller and less seasoned Washington leadership.

The principal reasons for pursuing the ICF program are that, (1) its scientific success seems likely and hinges on quite different physics than does that of MFE, and (2) the physical separation of the complicated and expensive driver mechanism from the target area is an attractive feature for reactor design.

The principal concerns are the energy/power requirements of the driver and the energy efficiency of the system.

Considerable progress has been made in the last few years toward a scientific feasibility experiment utilizing glass lasers, which are amenable to early and meaningful feasibility experiments but which have limited efficiency. Much less progress has been made toward providing the data to evaluate more efficient drivers and their target coupling characteristics.

The program consists of the following: (1) a large comprehensive project at Lawrence Livermore Laboratory (LLL) which is principally aimed at obtaining significant thermonuclear burn as soon as possible utilizing glass lasers (40% of total ICF program). This project includes calculational work on targets for several drivers, considerable diagnostic development, and a small effort in systems analysis and advanced laser development; (2) a large project at Los Alamos Scientific Laboratory (LASL) aimed at defining the potential of CO_2 lasers for inertial confinement fusion including CO_2 laser target design, and a small effort in systems analysis (21%); (3) a project at Sandia Laboratory (Albuquerque) aimed at the use of intense beams of high energy electrons and light ions to drive inertial confinement experiments (8%); (4) two other smaller experimental programs on inertial confinement using glass lasers (14% total); (5) an advanced laser technology program (10%); (6) a research program mainly involved with system studies and the development of ion beams (7%); and (7) less than 1% for long lead technology.

1. Both LLL and LASL need strong comprehensive target design capabilities to support their present programs. However, the target calculation, fabrication and diagnostics at LASL are not now adequate to support exploration of the CO_2 laser option. These should be strengthened as a matter of high priority which, in our view, is more a matter of management than of additional money.

   The LLL target design group which is doing excellent work in support of the ICF program has evaluated a range of possible drivers. However, we think that it is unwise to have only one group working on such a central problem and recommend that a second group be established which considers the full spectrum of possible drivers and associated targets. In view of their potential capabilities, the classification problem, and our recommendation in the previous paragraph, LASL is the appropriate place to establish this second group.

2. Both the LLL and LASL experimental programs with Shiva and the Eight-Beam System can go a long way toward evaluating the promise of larger, more expensive facilities. These experiments should be pursued as a matter of high priority. A portion of any funds that are added to the programs of the two laboratories should be devoted specifically to expanding these current investigations.

3. A facility larger than the Shiva or Eight-Beam System will be required to drive high gain targets. Both laboratories have begun the design of such facilities (Nova and Antares). Since there may be problems with coupling high power to targets (more so with Antares than with Nova), we have two recommendations: (1) Decisions to commit further significant funds to these facilities (aside from relatively small-cost, long lead items) should be delayed until the results of coupling experiments on the present facilities are in hand; and (2) in any event, such facilities should be designed so as to have the potential of growth to higher energy, if the growth is warranted.

4. The eventual feasibility and cost of the program depends on the driver energy at which high gain can be obtained from the target, which is, in turn, governed by the combined efficiencies of driver plus implosion geometry. Since all presently known lasers start out with relatively low efficiency, it is necessary to investigate other drivers. It is not sufficient to investigate only the technology of those drivers. The

drivers and implosion geometries must be investigated as systems. This part of the program needs considerable strengthening. Such investigations require careful calculations and diagnostics, including diagnostics of the details of the coupling mechanisms. The work should be carried out where the tools, expertise and support exist.

5. The work at Sandia includes a set of approaches that are promising from the point of view of overall efficiency, but its underlying science is particularly complex. The work is valuable, but considerations of driver requirements and target coupling are not well integrated. This warrants a special review by a group that would include experts on the specifications on driver requirements and the technologies to be utilized. Management steps should be taken to ensure proper program integration and balance.

6. The DOE laboratories, LLL, LASL, and Sandia, will continue to be national assets in this area, regardless of whether or not their present drivers are appropriate for the long term. Consequently, the program will benefit in long-term potential if these laboratories participate in the national program on other drivers, including but not limited to, advanced lasers.

7. Present classification policy may impede adequate assessment of new drivers and coupling systems at places other than the weapons laboratories. The classification policy should be reviewed now and on a periodic basis to make sure that no unnecessary hindrance is placed in the way of the program and that information which should be classified is properly protected.

V. Program Management

Implementation of the program strategy discussed above will require a more integrated headquarters management than has been necessary in the past. This need occurs because the recommended strategy must be implemented by efforts across a broader technical front involving more participants, and still provide timely data to support key program decisions.

The degree to which the management of the inertial and magnetic confinement programs should be combined depends chiefly on what is judged to be the primary motivation for the inertial confinement program. If the weapons technology applications of ICF are dominant,

then clearly the program management should remain separate as it is now. Should commercial energy rather than weapons technology application become the driving factor behind the ICF program, it would be preferable to have both ICF and MFE under common management. Even though the requirement for common management is not pressing in the near term, it does make a great deal of sense to develop, perhaps through the Fusion Review Committee, integrated program objectives, strategy and a coordination of critical milestones, since the potential of MFE and ICF for fusion energy should be judged on a common basis. Consequently, the pooling of data and cost information for reactor studies, and the establishment of common measures of performance is essential.

In addition to the above, we recommend the establishment of a unified fusion coordination committee involving the program leaders from the laboratories, external experts, and headquarters management to provide a continuing program review.

An explicit program participation policy should be developed clarifying and communicating the roles and responsibilities of headquarters, the DOE field offices, the national laboratories, industry, and universities. Because of the history of the relationship between the weapons laboratories and headquarters, some confusion exists and widely differing perceptions of what these roles should be are expressed by the various participants in the program. Clearly headquarters should be responsible for development and approval of program objectives, strategy, and plans, the monitoring and evaluation of results, and the presentation and defense of the program within the Department and to OMB and Congress. In carrying out these roles, the headquarters team should, of course, make extensive use of the technical expertise of the program participants. Where technical disagreement and competing views arise among the program participants, it should be the responsibility of the headquarters group to force the resolution of the issues by providing the forum for technical argumentation.

It was clear that such a headquarters group is established and functioning well in the magnetic fusion program. The equivalent group in the newer inertial confinement program is now evolving. The recommended strenthening of a second across-the-board center-of-excellence in the ICF program as well as involvement of other program participants in major subsections of the program will provide the basis for maturation of the proper headquarters role here.

While it was appropriate for the major activities in both programs directed toward the near-term scientific feasibility milestones to be pursued by the DOE laboratories, in light of the suggested strategy, the program would be strengthened by encouraging further participation by industry, universities, and the utilities. Where the DOE laboratories have unique capabilities, expertise, or facilities, or where control of classified information is essential, as in certain aspects of the ICF program, then the laboratories must play a dominant role. However, aggressive efforts should be made to take advantage of outside capabilities and potential wherever they exist to broaden and strengthen the scientific, technological and industrial base of the programs.

VI. International Cooperation

Since 1958, the magnetic fusion effort has been an exemplary one in terms of scientific cooperation among many nations. During this period, the U.S. has contributed its full share to this cooperation, contributing to international meetings and carrying on extensive scientific exchange programs with many countries, including those of Europe, the USSR, and Japan. This collaboration has been beneficial; many ideas have flowed back and forth which have significantly advanced progress. As we approach engineering test facilities and engineering prototype reactors whose expense will be large, more substantial collaboration may be desirable. The U.S. should continue to play a leading role in such cooperation. New initiatives, of course, should await the clear establishment and support of the direction of the U.S. program.

VII. Conclusions and Recommendations

1. Objectives

Observation:

There has been and continues to be considerable debate over the objectives and urgency of the fusion effort, the objectives and urgency of the nation's overall energy needs and the coupling of the two.

Recommendations

For the potential of fusion to be effectively determined over the coming years, an overall Departmental policy and consistent set of objectives should be established.

The objective of the programs should be to determine the highest potential for a commercial fusion energy source at the earliest practical date. To this end, a number of approaches to producing a reacting plasma must be evaluated, difficult engineering problems must be solved, the best combination of scientific and engineering promise must be selected, and the cost of a practical system must be determined.

Demonstration of scientific and technological feasibility should remain the near-term aim of the program. Its achievement should be a necessary, but not sufficient, step in the decision to proceed with the construction of an engineering prototype reactor. That decision should include the evaluation of the suitability of the various contending approaches for a reactor as well as the attainment of required technology.

2. Strategy

Observation:

The present fusion stategy is characterized as high risk; it emphasizes carrying the front running approach as quickly as possible to a demonstration of commercial feasibility while maintaining a vigorous backup program.

Recommendations:

Adopt a modification of strategy that reduces the risk by broadening and strengthening the technical base from which to choose the best fusion approach to practical energy production. This will surely require the commitment of significant funds within the next few years for facilities to obtain engineering and materials data as well as achieving scientific feasibility.

Pursue vigorously several physics approaches and carry out in parallel, engineering and materials test programs until at least one potentially economically competitive design is identified.

Lay the groundwork for one or more engineering test facilities to be committed as soon as practical after the potentially competitive designs are identified.

3. Programs

a) Magnetic Confinement

Observation:

The program has been successfully following a self-consistent logic supporting an internally generated objective and timing. With the recommended modification in strategy, there are associated changes in emphasis.

Recommendations:

Evaluate the critical physics and engineering questions, both individual and coupled questions, in Tokamaks and mirrors.

Obtain a thermonuclear burn in Tokamaks as quickly as practical. However, commitment to construction of a next generation Tokamak beyond TFTR, should not be made until results from TFTR and other related experiments justify it. A concern of the Group with the Tokamak concept was its apparent complexity as a reactor. Before commitment to a Tokamak-type engineering prototype reactor (an integrated energy-producing system), a convincing case should be made that Tokamaks can be engineered into attractive energy producers.

Select and support a few of the most promising alternative approaches to provide a real test of the concepts.

Observation:

Discoveries in plasma physics are still a pacing item in the magnetic fusion program as is demonstrated by numerous recent findings.

Recommendations:

Operate, modify, and exploit current facilities, both existing and under construction, to the greatest degree practical to accelerate the flow of essential physics and engineering information.

Insure a strong plasma physics foundation to support the fusion program; additional support should be made available, perhaps through the Office of Basic Energy Sciences.

b) Inertial Confinement

Observation:

The inertial confinement program is in a phase of rapid development with many driver-target options. This rapid change and the complications of classification form the basis for a set of specific suggestions.

Recommendations:

Operate, modify, and exploit current facilities, both existing and under construction to the greatest degree practical to obtain essential physics information.

Develop at LASL a fully competitive target design, coupling, and fabrication program.

Base further large commitments to planned facilities on formal reviews of the experimental results obtained with existing facilities.

Pursue the development of alternative drivers which offer the potential for achieving performance parameters required for eventual commercial use taking into account target coupling.

Review the objectives and technical status of the e-beam program at Sandia. Take management actions to assure program integration and balance.

Review the current classification policy, its impact on the program, and the protection of information which should be classified.

4. Management

Observation:

There are many participants in the fusion program management process with different perceptions of the various roles. When this complexity is coupled to the fact that the two fusion

programs have developed both separately and from different starting points in time and origin, there appear to be many areas for fruitful action.

Recommendations:

Implement a coordinated management of the MFE and ICF programs:

- o Develop a common basis for establishing program planning, a data base and ongoing evaluation.

- o Establish a unified fusion coordination committee involving the program leaders from the laboratories, external experts, and headquarters managers to provide a continuing program review.

Clarify, develop, and communicate the responsibilities and authorities of headquarters, laboratories, and the field offices, and develop the necessary competence in each group.

Expand and evolve appropriately the participation of universities, industry, and users as the program develops, e.g., in the areas of:

drivers/heaters;

systems engineering;

alternate concepts/drivers; and

physics investigations;

5. Items Not Fully Addressed

Observation:

In the five days of briefings and deliberations, it was simply not possible to deal adequately with every major issue in the complex fusion programs. However, four items have been identified as requiring a prompt and dedicated review.

Recommendations:

Determine the role of the fuel producing fusion hybrid in the overall fusion program.

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Determine the potential changes in objectives of and limitations on the ICF program in the event of a comprehensive test ban treaty.

Conduct an in-depth review of the scientific and technological status of the fusion programs.

Determine the impact and potential of international cooperation on the development of the U.S. fusion program and recommend appropriate actions.

GLOSSARY

Fusion research, development, and commercialization can be divided into two principal stages:

Stage 1 - The determination of the potential of fusion as a practical source of energy (government as the primary sponsor); and

Stage 2 - The commercialization of fusion as an energy option (private sector as the primary sponsor)

Within Stage 1, there are two phases -

Phase 1 - Achievement of scientific feasibility and development of the foundation for Phase 2.

Phase 2 - Sequential achievements of technological and engineering feasibility.

Scientific Feasibility - Achievement of a reacting plasma in which fusion energy released exceeds the energy input to the plasma - is expected to require facilities such as TFTR/TFTR Upgrade, post-MFTF, Nova and Antares.

Technological Feasibility - Achievement of an elementary integrated system producing usable energy - is expected to require one or more engineering test facilities in which each of the technological requirements for a potentially economically competitive design has been met with at least the basis of a workable solution.

Engineering Feasibility - Achievement of an integrated system producing net usable energy in a manner indicative of an economically competitive design - is expected to require an engineering prototype reactor in which each of the technological and engineering requirements is met with a solution transferable to commercial designs.

Economic Feasibility - Achievement of an integrated system producing net usable energy in an economically competitive manner - is expected to require a commercial demonstration reactor in which each requirement is met with an economically competitive solution.

FEB 27 1978

Dr. John S. Foster, Jr.
Vice President, Energy R&D
TRW, Inc.
1 Space Park
Building R4-2004
Redondo Beach, CA 90278

Dear Dr. Foster:

The U. S. Department of Energy, through its Research and Development Coordination Council, intends to convene an Ad Hoc Experts Group to conduct a concise review of DOE's total fusion energy program, including both the magnetic confinement and the inertial confinement fusion programs. This will be an initial step toward establishing a DOE position on U. S. fusion energy research. As Chairman of the Fusion Review Committee of the DOE R&D Coordination Council, I am very pleased that you will be able to accept the invitation to organize and chair this Ad Hoc Experts Group.

The objective of the Ad Hoc Experts Group will be to review and present recommendations regarding the content and balance of the DOE fusion programs. For this purpose, the group will meet on March 14, 15, and 16, 1978 in Room 8222C, 20 Massachusetts Avenue, N. W., Washington, D. C. After a short Executive Session, the group will be briefed in some depth by the MFE and ICF Headquarters staff and by key participants from the national laboratories. Approximately one day will be allocated to each of the two programs, divided approximately equally between briefings and discussions. The third day will consist of a half-day of discussion followed by a half-day of drafting a brief written document. DOE staff will be available to assist in the documentation if the Group so desires. A list of Group members, a preliminary agenda for the meeting, and a list of some of the questions to be addressed are enclosed. We recognize that authoritative answers to all these questions will not be possible in the brief time available for this review. Additional background material will be forwarded to all members under separate cover.

I look forward to seeing you on March 14, 15, and 16. I have designated Dr. Robert A. Summers of my staff as my personal representative to work with you in carrying out this endeavor. He may be reached at the Office of Energy Research, Division of R&D Coordination (202/376-4051).

Sincerely,

SIGNED BY JOHN M. DEUTCH

John M. Deutch, Director
Office of Energy Research

Enclosures:
As stated

Department of Energy
Washington, DC 20585

October 5, 1989

The Honorable Wendell H. Ford
Chairman
Subcommittee on Energy Research
and Development
Committee on Energy and Natural
Resources
United States Senate
Washington, D.C. 20510

Dear Mr. Chairman:

On June 14, 1989, Dr. Robert O. Hunter, Jr., Director, Office of Energy Research gave testimony before your subcommittee to discuss magnetic fusion research, and development and demonstration programs.

Following that hearing, Ranking Minority Member James A. McClure submitted written questions for our response to supplement the record. Enclosed are the answers to those questions, which also have been sent to Senator McClure.

If you have any questions, please have your staff call Frances M. Bryant on 586-4277. She will be happy to assist.

Sincerely,

Joseph C. Karpinski
Principal Deputy Assistant Secretary
Congressional and Intergovernmental
Affairs

Enclosures

POST-HEARING QUESTIONS AND ANSWERS

RELATING TO THE

JUNE 14, 1989 HEARING

SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

OF THE

COMMITTEE ON ENERGY & NATURAL RESOURCES

UNITED STATES SENATE

WITNESS: DR. ROBERT O. HUNTER, JR.

DIRECTOR

OFFICE OF ENERGY RESEARCH

UNITED STATES DEPARTMENT OF ENERGY

QUESTIONS FROM SENATOR McCLURE

Magnetic Fusion Energy

Question 1: What is the overall objective of your new policy with respect to the fusion energy research program?

Answer: The overall objective of the proposed new DOE fusion policy is to develop the highest potential for employment of fusion energy. The DOE views the fusion program as one of great significance as a potential "inexhaustible" energy source for the next century. The Department thinks that a statement of the advantages of fusion remains very exciting.

First, the principal raw materials for fusion fuel, deuterim and lithium, are extremely abundant and inexpensive natural resources.

Second, fusion reactors would be inherently safe against the possibility of catastrophic accidents, in the sense that runaway reactions could not occur. The reactions that produce fusion energy are immediately quenched without considerable external support so continued reactivity and heating of fuel or other elements of the reactor is simply not a prospect. The major difficulty of creating fusion reactions--that they do not happen naturally in the fuel we

Answer 1 continued: will supply--becomes, in this sense, an advantage. And third, the fuel cycles and operations of fusion reactors would be environmentally highly attractive. Although some radioactivity is certainly involved in fusion, it is easier, overall, to deal with than the radioactivity of fission fuels, reactors, and wastes.

Question 2: What were the guiding principles that led to your decision to redirect the fusion program?

Answer: The guiding principles of the new proposed DOE fusion policy are:

a) to focus effort on the critical physics issues throughout fusion R&D;

b) to provide a strong competition in R&D within and between the magnetic and inertial fusion approaches;

c) to establish specific milestones, close to the turn-of-the-century, for the ignited CIT device in the magnetic program and the high-gain LMF device in the inertial program; and

d) to practice fiscal responsibility.

QUESTIONS FROM SENATOR McCLURE

Question 3: How will the universities, DOE laboratories, and the basic researchers fit into this new direction?

Answer: The Department's program will continue to require strong involvement from universities, DOE laboratories and basic researchers. The university programs provide much of the depth in basic plasma physics research that guarantees the scientific strength of the program as it moves forward; the DOE laboratories provide the institutional base for large research enterprises. Clearly, however, both groups play the opposite roles as well (for instance, the basic research at LLNL and the programs at MIT, Rochester and Princeton). The Department also wishes to encourage more active involvement of industry in fusion R&D, and the competitive aspects of the policy should increase the participation of industrial researchers.

Question 4: Have you or will you subject this new policy direction to review by the National Academy of Sciences or by some other outside peer review group such as your own advisory committee?

Answer: Yes. The proposed new policy will be subjected to thorough review by a panel of independent experts reporting directly to the Secretary.

QUESTIONS FROM SENATOR McCLURE

Question 5(a): Have you sought any outside expertise to help you identify the essential physics elements and experiments needed in your newly focused program?

Answer: Yes. The Magnetic Fusion Advisory Committee, through its expert panel on Confinement and Machine Physics, provided a detailed discussion of issues, including experiments and theory required to develop a predictive understanding of tokamak transport. The National Academy of Sciences (NAS) recently completed a study of magnetic fusion policy, chartered by the Department; the NAS also studied inertial fusion in 1986, and has recently begun another study of it. All those sources, plus a number of independent academic and government scientists (both within and outside the fusion programs) have been and will continue to be consulted.

QUESTIONS FROM SENATOR McCLURE

Question 5(b): How confident are you that these physics experiments will enable us to scale-up fusion machines with any more certainty that we can now using our current, essentially "empirical" knowledge?

Answer: Because of the development of new diagnostics for magnetic fusion, it has become possible to measure small-scale fluctuations and correlate their magnitude with the transport of energy out of the tokamak. This was first done at the U. of Texas and at ORNL. At PPPL, new measurements have made it possible to determine details of transport in the center of the plasma. Additional new diagnostic techniques, such as far infrared laser scattering, beam emission spectroscopy, and microwave scattering are being developed and applied to our operating tokamaks.

In the Defense Program's inertial fusion research activities, there have been recent experimental results on laser-pellet interaction that agree well with the predictive computer codes. There have also been encouraging new classified results.

I am, therefore, highly confident that we will gain a physics-based understanding of the critical processes in both magnetic and inertial fusion, and that this

Answer 5(b) continued: understanding will permit us to construct ignition/high-gain devices that will meet their fundamental objectives. I do not believe that we can afford to proceed with such large investments on the basis of phenomenological predictions alone.

Question 6: What specific work is necessary to set the stage for the next fusion machine?

Answer: The specific work required for the next magnetic fusion device, the CIT, is to obtain a predictive, physics-based understanding of the processes dominating energy confinement in tokamaks. Specifically, it is necessary to measure the fluctuations in the tokamak plasma and correlate them with the transport of energy out of the plasma. An adequate predictive capability would explain current experiments within a factor of about 2 and would allow the confident design of an ignition experiment.

The specific work in inertial fusion involves both science, in the target physics, and technology, in the driver development. The principal target physics issues are: behavior of fluid instabilities as capsule drive energy reaches the inertial fusion highgain range of ten megajoule or less, and the interaction and coupling of laser light in target enclosures of the appropriately small dimensions characteristic of this ten megajoule or less drive energy. Driver development work requires demonstration of scalable components of glass and excimer lasers that can meet inertial fusion energy and power requirements at reasonable

Answer 6
Continued: cost, demonstration of focusing capability for light ions, and, for heavy ions, of beam handling and combining with a small spot size.

Question 7: Are the ICF and magnetic fusion programs both at a comparable stage in their development so that they can effectively compete in a "horse-race" by the year 1992 for the selection of a single technology for an ignition machine? What effect will a sudden switching of gears on our part do to the commitments we have already made in the international fusion community?

Answer: The proposed policy does not suggest that ICF and MFE compete for a single ignition experiment. Rather, both programs construct the ignition devices, the CIT and the LMF, that they are already planning for as their next major experiments. Thus, no sudden switching of gears is proposed. The direct competition between the two approaches to fusion does not occur until after the turn-of-the-century, when the performance of the ignition devices has allowed us to evaluate their relative potential for commercial power production. Whether the single engineering test then constructed to study reactor engineering issues is done internationally or unilaterally is not specified; thus, there is no need to change radically any of our international commitments or cooperative programs.

QUESTIONS FROM SENATOR McCLURE

Question 8: Why have the ICF and magnetic fusion programs to date followed such separate, independent paths? Why hasn't the ICF concept been looked at before now as a realistic alternative to magnetic fusion in terms of an energy-producing technology option?

Answer: The magnetic fusion energy program is an older and more mature program than ICF, and considerably more has been spent on magnetic fusion energy. However, the 1978 Department of Energy Policy for Fusion Energy did propose that the two fusion programs be competed for their energy applications. This policy was not actually followed. Insofar as ignition is concerned, recent, very promising advances in inertial fusion science and technology now suggest that the program stands, relative to its LMF device, in roughly the same position as magnetic fusion, relative to its CIT device. However, the issues relating to reactor design and driver development for the energy application will have to be explored in addition to the ignition experiment.

Question 9: I have heard some rumors to the effect that part of the reason for the shift to ICF research may be related to personal gains that Mr. Hunter could realize. Do you care to comment on these allegations?

Answer: I very much appreciate the opportunity to comment publicly on these slanderous allegations, since the people who actually made them have chosen to remain anonymous. Before I entered the government, I sold my company in order to divest myself of any assets that could be construed as remotely related to DOE programs of any kind. I hold no such assets or financial interests. As far as I know, I have no patents with any financial value whatsoever, related to DOE research or otherwise.

APPENDIX II

Additional Material Submitted for the Record

Cornell University

Sibley School of
Mechanical and Aerospace Engineering
Upson and Grumman Halls
Ithaca, New York 14853-7501

16 June 1989

Senator J. Bennett Johnston
Chairman, Committee on Energy and
Natural Resources
United States Senate
Washington, D.C. 20510-6150

Dear Senator Johnston:

Ref: Hearing on the Department of Energy's Magnetic Fusion Energy Research, Development and Demonstration Program, June 14, 1989

With your permission I should like to submit the following written comments for the record. At the hearing referred to above, you asked both Dr. Dan Dreyfus and me why it should take so long to commercialize fusion energy once its scientific feasibility has been proven. This is an exceedingly important matter and I am afraid neither Dr. Dreyfus nor I gave you an adequate answer. Nor does the NRC committee report on which we testified go into details on this point.

I can tell you, however, that the NRC committee reached its conclusions concerning time scales largely by drawing analogies with the history of the nuclear industry and other large scale energy programs. According to this line of reasoning, I would proceed as follows.

If fusion is to be a commercial reality by the year 2050, let us say, then I would expect it to be producing enough electricity by then to account for a few percent of the total amount of electricity being generated nationally. That is what the nuclear industry accomplished by the mid 1970s, some 20 years after Shippingport. This would imply, by analogy, that by the year 2030 or thereabouts a demonstration fusion reactor would have been built and operated; and that one or more prototypical fusion power plants would have been built and operated after the year 2030 to finally reach the point where fusion energy was shown to be competitive, with other alternatives.

As our committe has pointed out, the Department of Energy's magnetic fusion energy program plan does not now include a demonstration plant or the details of a strategy for commericalization. An ITER type of test reactor costing several billion dollars in not a suitable model for the design of a commercial reactor. ITER would be a necessary step in the engineering development phase of the program and would have to precede any demonstration plant by a number of years.

Thus, the extended timetable begins with scientific experiments to show that a plasma can be ignited and that it can be contained while it is burning. If this can be achieved by the year 2000 or soon after, then it may take another 10 to 15 years to complete the engineering phase with the help of an ITER-like facility. By then we have reached the year 2020 and that may be th earliest when one would commit to building a demonstration reactor, a DEMO that migh: not be ready much before 2030. And this would be followed by the steps mentioned earlier, on the way to a final commercial product.

I do not wish to imply by this timetable that one could not speed up the development and testing process by taking some risks and cutting some corners. The reasoning that I have presented here may be somewhat conservative, but I think it is reasonably consistent with the earlier experience of the nuclear industry. That experience may be the best guide we have now for laying out the long term strategy for fusion energy.

Sincerely,

Peter L. Auer
Professor

PLA:jss

cc: D. Dreyfus

UNIVERSITY OF WASHINGTON
SEATTLE, WASHINGTON 98195

College of Engineering
Department of Nuclear Engineering

June 7, 1989

Dr. Robert O. Hunter, Jr.
Director, Office of Energy Research
U. S. Department of Energy
1000 Independence Avenue
Washington, DC 20585

Dear Dr. Hunter:

The Magnetic Fusion Advisory Committee (MFAC) met in San Diego on June 6-7 to consider the Panel 22 Report on Confinement and Transport, Machine Physics and CIT Ignition, as charged in your letters of December 3, 1988 and February 3, 1989.

In the San Diego meeting MFAC also heard a summary of the recent NAS/NRC study on the magnetic fusion program. This report recommends that fusion play an important part in a US energy plan for the next century, and that a timely start for the CIT is an important next step.

Panel 22 worked under a very broad charge in a short period, producing an excellent review of the status of the transport issue. We note that the analysis of the transport picture closely parallels that of the Transport Task Force (TTF). MFAC reiterates the support of the Panel 22 for efforts expended in the TTF. MFAC accepts the report enclosed herewith, but provides below our own assessment of certain issues addressed in the report.

1. While transport is an important issue and is properly being given high priority, it would be a great mistake to focus unduly on this issue at the expense of other program elements important for fusion development or to use it as a sole criterion for evaluating program activities.

2. In attempting to identify resources for the transport initiative, the Panel on several occasions identifies and questions continuation of facilities that it sees as not

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contributing to transport. This is in spite of considerable program investments in facilities that are close to completion, and which have capabilities of doing valuable fusion physics. Examples of this include the D-T option in TFTR, CPRF/ZTH, the FRC program, and to a lesser extent MTX. These programmatic decisions must be reviewed in the context of the full fusion program.

3. Fusion progress, like almost any scientific development effort, will proceed best when both empirical and theory driven means are used in conjunction. The Panel 22 report underemphasizes contributions that can be made by empirical science.

4. The Panel report calls for specific reorganizations of MFECC Computer Center activities. These recommendations reflect the changes that are occurring in large scale computing, associated with questions dealing with the efficient use of existing and newly acquired resources. Several studies, both inside and outside fusion, are examining these questions. Although MFAC expects that some reorganization will prove beneficial, it recommends not acting prematurely until this issue has been more thoroughly examined and until changes can be made in a well-coordinated way throughout the program.

5. In dealing with the question of tritium in TFTR, the NRC report arrived at the same position as that of MFAC Panel 17. Specifically, assuming the CIT is to be built, D-T operation in TFTR would provide valuable experience of tritium handling in an operating tokamak. The Panel 22 report implicitly discounts this value by dismissing tritium operation as not important for transport studies.

The D-T period in TFTR has been delayed by about three years by the transport initiative. During this time, TFTR will be making important contributions to the transport activity. In light of the concerns raised by the Panel, MFAC recommends a review of the costs of the TFTR D-T option. However, MFAC attaches high priority to maintaining this capability.

6. In questioning the value of the descoped ZTH facility, the Panel focusses on its 1.7-MA capability as a single figure of merit, rather than addressing the physics contribution that the facility would make. MFAC reaffirms the value of the RFP research program and notes the substantial resources already committed to the CPRF/ZTH construction. We support the review suggested by Panel 22 and recommend that it focus on the effectiveness of the facility in producing important physics results at both the lower and higher operating currents. We also note that when considering the roles of RFX and ZTH, the existence of comparable but complementary facilities is most valuable in developing the underlying science.

7. We strongly reaffirm our comments relating to CIT contained in the MFAC letter to you of March 9:

> "In spite of the uncertainty in predicting ignition, the Panel endorses the CIT concept as the proper next step for the program; and, as the general parameter range for the machine is known, it endorses continuation of the necessary design and R&D for the machine. MFAC likewise continues its support for the construction of CIT as expressed on a number of earlier occasions. This will require additional funds in future years.
>
> "The transport initiative will reduce some of the uncertainties regarding ignition. Proceeding at the same time with the construction of the CIT with its basic capability will position us to exploit fully the results of the transport initiative in the experimental phases of CIT."

In the subsequent meeting, Secretary Watkins expressed his support for an ignition experiment within ten years and the need for a physics checkpoint on the transport issue. In response to the sentiments he expressed, we believe that progress on the remaining transport questions will be adequate within the next few years to provide a basis for ignition; that the CIT design, R&D, and prototyping should proceed in parallel with the transport work; and that the check point should release construction funds for the project, rather than initiate them.

Because of the interest in the fusion program expressed by the Secretary, along with the potential sweeping impact of the Panel 22 Report and the foregoing recommendations on the program, I request that I discuss these findings with both you and the Secretary.

Sincerely yours,

Fred L. Ribe

Fred L. Ribe
Chairman
Magnetic Fusion Advisory Committee

FLR:shk

Enclosure: Report of MFAC Panel 22: "Confinement and Machine Physics"

The report "Confinement and Machine Physics" is printed following Dr. Baldwin's statement.

June 14, 1989

Senate Energy Committee
Hearing on Magnetic Fusion

Honorable Members:

I feel the fluid and form parameters in the enclosed Theory of Natural Propulsion outline magnetic fusion. Our research team has consistently noted a magnetic anomaly at higher efficiency levels of flux of form processes. This relates to ergodic theory if topography is saved for a measure preserving flow. Further symmetry has been obtained in all areas covered to date.

Thank you for your consideration.

Carl Henry Hollingsworth

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A THEORY OF NATURAL PROPULSION

The key to natural propulsion lies in the duality of compounded matter. Both two-and three-dimensional properties seem to coexist in the same space. Time and its motion are intertwined in the problem. Albert Einstein demonstrated that time, by virtue of its property of motion, is equivalent to a spatial dimension. Three spatial dimensions (height, depth, breadth), in combination with time, constitute four dimensional spacetime. However, difficult it may be to visualize four dimensions, experimental results proving this concept are not in doubt. A theoretical model incorporating the elements of four dimensional spacetime into a three dimensional construction follows.

Let's describe compounded matter as a two-sided affair. Both sides occupy the same space, with the maximum number of dimensions to each side being three. Since we observe time, one side must comprise two spatial dimensions with time's motion as its third dimension. In the same vein, the second side must have three spatial dimensions with no motion.

We need to consider this two-sided model's geometry in order to understand this concept.

After observing soap films stretched across wire figures, Felix Plateau suggested possible stable configurations of the latter, motionless, side were the plane, the sphere or any portion of a sphere. They can be described as minimal in respect to volume enclosed or as surfaces of constant mean curvature. These motionless environments need to stable only to the extent that a disturbance will readjust itself. More accurately, the total balance of all disturbances will leave the spherical, or plane, surface as it was before. With the readjustment of disturbances we come to the second part of our description. The motion of these disturbances can be viewed as time. This leaves room for only two spatial dimensions in that side.

The geometry of this is explained by George Reimann's two-dimensional theory. At the time of the theory's conception it was labeled as an abstract exercise. Since then it has become the basis for cartography and navigation. According to Reimann, on a non-Euclidian surface, the shortest distance between two points is a curve. When curvature is zero, or constant, as in Euclid's three-dimensional rules, objects can move in space without changing their shape. In Reimann's rules, however, distortions will occur. So when we are considering nature's common factors, you need to look for obvious shapes and interconnections, but also for their two-dimensional distortions.

Examining this in relation to natural propulsion, a consistent pattern emerges for animated things. The pattern is a topographic distortion: a distortion plane of spiral curves, superimposed upon each other and sharing the same center. How do animated forms' two-dimensional distortion planes tie in with a force that aids propulsion?

The geometry of their shared center spirals offers a curious solution. The side with the two spatial dimensions, plus time, must follow the rules of the square. The other side, with three spatial dimension, must adhere to the rules of the cube. By definition, a spiral curve must keep its center or lose its history of progression. The difference between two-thirds gnomic progression (on the squared side) and one-third progression (on the cubed side) seems to create the geometric propulsion.

To understand how one side can push or pull the other side, one must realize that both sides occupy the same space. Three-dimensional matter can exist in the spiral planes of animated form in association with the two-dimensional side. It exists in a sense, as a projection.

An innovative researcher, David Bohm, has outlined this projection possibility in his theory describing the universe as a holographic unfolding. A hologram is a good analogy. When any portion of the whole is illuminated, the entire image will be reconstructed. Photons of light would be the illuminators of the projection, lending substance to reality. Neutrinos would illuminate three-dimensionality, producing the holographic image. The smaller the portion of the whole chosen for illumination, the less detail passed on to the projection, or the narrower the range of focus.

Getting back to the force that aids propulsion, we need to consider the common factor of viscous fluid interactions with animated form. Looking at fluid dynamics, there are two kind of activity in relation to a surface: turbulent flow and laminar flow. Turbulent flow is just as it sounds, it moves past its associated surface in roiling eddies. Laminar flow occurs when a fluid bands with its adjoining surface moving in a smooth uninterrupted stream. Laminar flow is not the only factor at this point. The laminar flow has created a boundary layer between itself and the surface. Neither before the fluid achieves banding nor after the transition to turbulence does this boundary layer exist.

The main variables in determining conditions suitable for achieving laminar flow are curvature, chord and skin friction of the associated surface in relation to the density and speed of the fluid. When laminar flow stabilizes, the boundary layer exhibits some decidedly odd tendencies. These include movement perpendicular to the flow and toward deepening chord depth. It seems when the boundary layer behavior concentrates at the point of deepest chord, the force aiding propulsion is triggered.

Banded fluid-animated form interactions, which I call flux of form, are nature's method of initiating a mass-energy transfer between form and the associated fluid. An example of that process is the dancing flight of the bumblebee. He uses his wings for mechanical flight, but also to orient the fluid surrounding his body into laminar flow. His form concentrates the boundary area behavior at point of deepest chord, which initiates mass-transfer from the bee to the boundary layer. A movement (relating to the two sides' difference in gnomic progression) then occurs. We observe it as sideways burst of flight. This turn of speed is also the limiting factor in the process, as it forces the transition from laminar to turbulent flow. Since the process had been tripped, mass-transfer is reversed and the boundary layer donates the transient mass back to the bee.

When the mass-energy transfer is initiated, it would appear to be a proton decay. When the cycle ends, it could be considered fusion with a gas by-product created in the disappearance of the boundary layer.

With this premise in mind, various flow models were constructed in an attempt to duplicate natural propulsion. A two-dimensional distortion of right-handed and left-handed equiangular spirals sharing centers was chosen for a central plane. A prime of two-thirds was selected as point of deepest chord depth. In the central plan two perpendicular lines intersect at the point of two-thirds the length. Spiral exterior form results in consistent progression of chord and curvature.

Initial tank tests were conducted using circular drum of water. After introducing a flow, models seemed to orient and maintain the flow. The first definite results were obtained using this tank and three feet of water. A model was constructed with two flux foils connected by a two-inch crossbar. It was thoroughly sealed to prevent absorption of water. Lead sheathing was applied to the crossbar of a sufficient weight to achieve neutral buoyancy. The model suspended three inches beneath the surface. While the model was suspended, a circular flow was introduced with a paddle. After a brief orientation, the model swam to the surface, achieving positive buoyancy. It then circled the tank, gradually losing speed. No observable buoyancy loss was noted at the slowest speeds. Upon stopping a negative buoyancy was observed as the model sank to the bottom of the tank. A mass transfer, of some sort, seemed to be demonstrated.

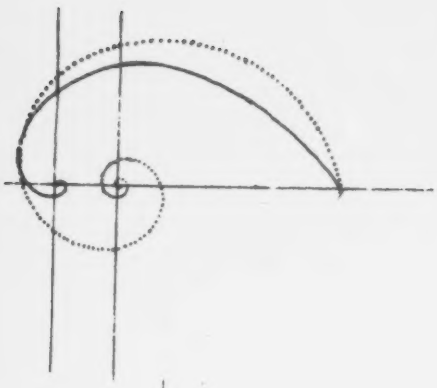
After several more prototypes were attempted, a configuration was found that produced a darting propulsion in response to a flow past it. The ideal testbed for this proved to be an ovalended tank with nearly straight sides. During these tests, gas bubbles were noted forming on the submerged portion of the model and tank. A flask was held upside-down in the tank. Gas bubbles were dislodged from the bottom into the flask. The sample tested to be enriched with carbon dioxide.

These flow tests were not intended as replicable experiments, but rather as a materialization of theory based on two-sided, three dimensionality and flux of form. As such, they should be considered theoretical results since they were approached but not realized.

In conclusion, the level of efficiency achieved, by any flux of form process, determines the initial spatial conditions of its associated chaotic system. We can further define this process, and the conditions surrounding it, as the most organized level of deterministic chaos; the prime attractor of all physical systems.

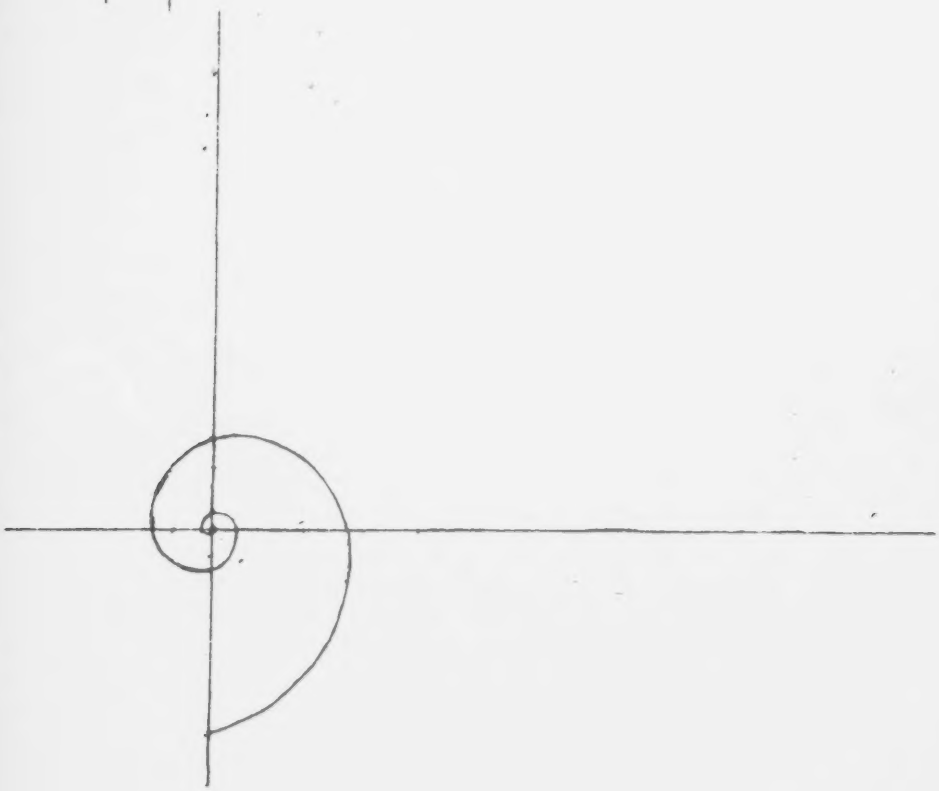
A new technology awaits its understanding.

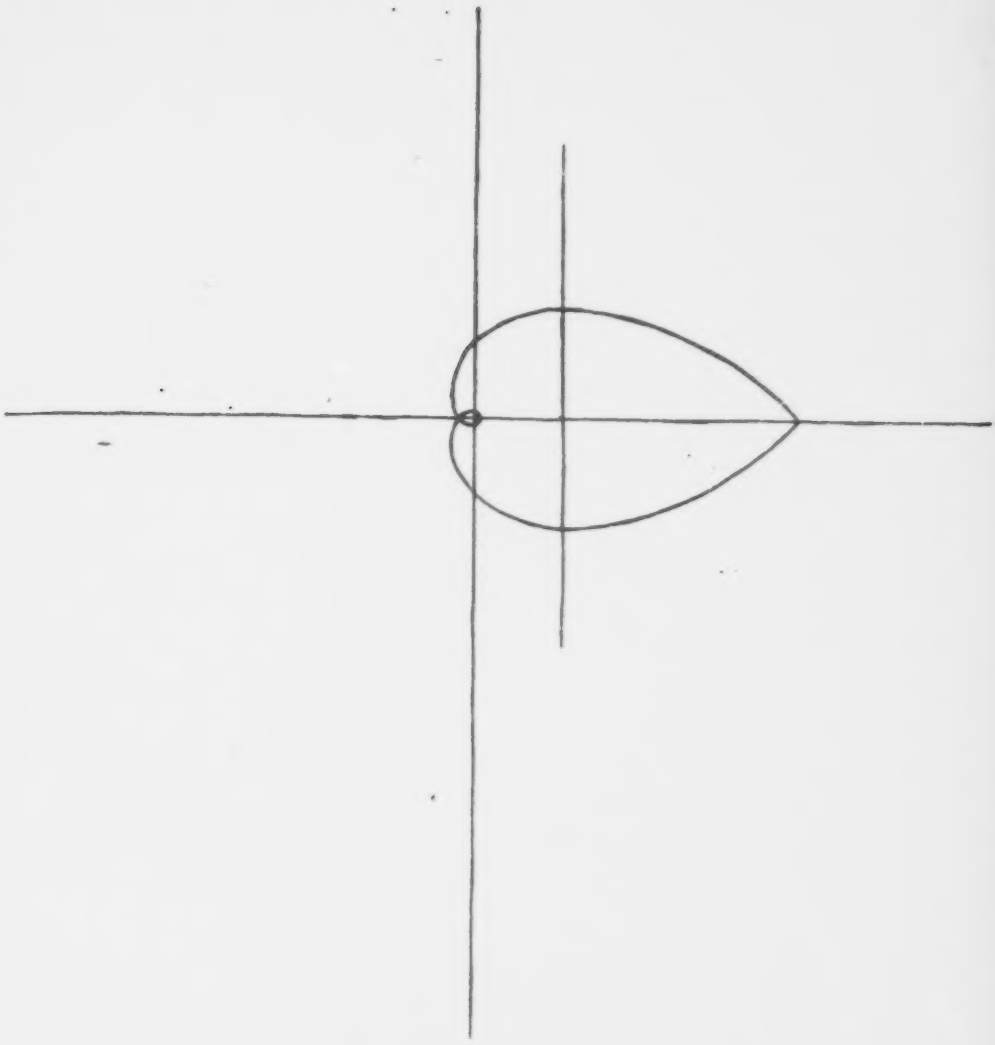
Carl Henry Hollingsworth



..... 2/3's geomic progression

—— 1/3 geomic progression

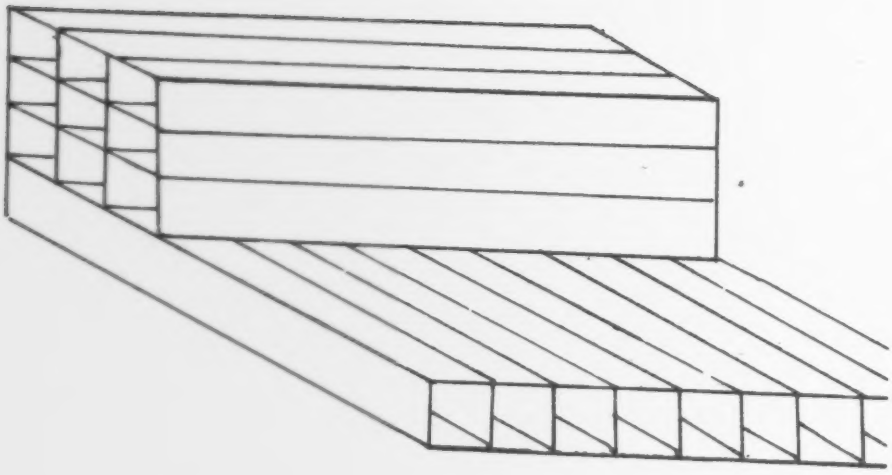




- equiangular spirals with greatest chord depth at 3/5 of the length and the breadth of form being 3/5's the length.

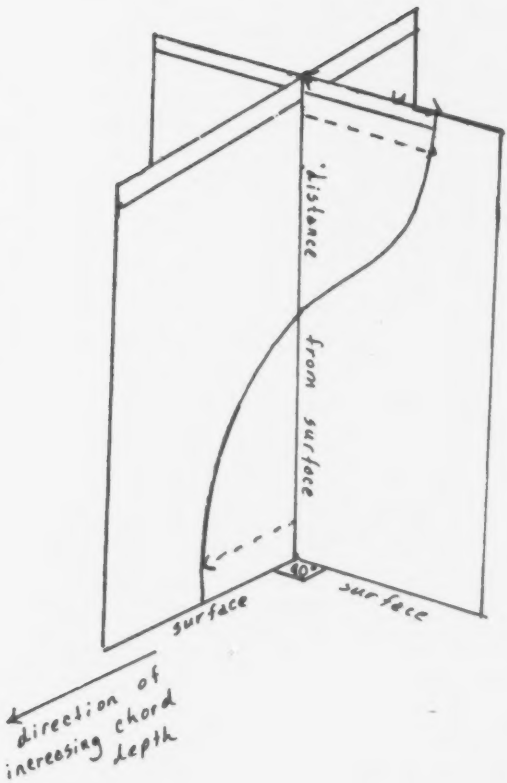
Is this what you want? See ¶

This is not the same spiral as either the 3/5 or 1/3 gnomic progression - the gnomic progression here is +/- 1/4



stacked streamtubes
(showing no changes in velocity)?

drawn without perspective



to show movement perpendicular to flow

I've graphed the boundary layer coming to rest then picking up speed - going at 90° to flow - using arbitrary velocity / depth ratios - what do you think?
how's it work?

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31. I am indebted to M. G. Turner, a research associate on the Georgia Project, which was supported by the W. K. Kellogg Foundation. Other members of the task force were E. O. Box, J. L. Cooley, F. B. Golley, J. E. Kundell, J. A. Little, V. Meentemeyer, D. Morrison, H. R. Pulliam, and R. Westmacott. I also acknowledge work of D. A. Crossley, D. C. Coleman, P. F. Hendrix, B. R. Stinner, G. J. House, and P. M. Groffman of the agroecosystem team, supported by grants from the National Science Foundation.

Ergodic Theory, Randomness, and "Chaos"

D. S. ORNSTEIN

Ergodic theory is the theory of the long-term statistical behavior of dynamical systems. The baker's transformation is an object of ergodic theory that provides a paradigm for the possibility of deterministic chaos. It can now be shown that this connection is more than an analogy and that at some level of abstraction a large number of systems governed by Newton's laws are the same as the baker's transformation. Going to this level of abstraction helps to organize the possible kinds of random behavior. The theory also gives new concrete results. For example, one can show that the same process could be produced by a mechanism governed by Newton's laws or by a mechanism governed by coin tossing. It also gives a statistical analog of structural stability.

ERGODIC THEORY AROSE OUT OF AN ATTEMPT TO UNDERstand the long-term statistical or probabilistic behavior of dynamical systems such as the motions of a billiard ball or the motions of the earth's atmosphere. The theory focused on certain mathematical objects called abstract dynamical systems or measure-preserving flows. The idea here is to abstract out the statistical properties and ignore other properties of the dynamical system. Thus two systems are considered the same when viewed as an abstract system (we call these isomorphic) if, after we ignore sets (or events) of probability zero, there is a one-to-one correspondence between the points in their phase spaces (see below) so that corresponding sets have the same probability and evolve in the same way (in other words, maintain the correspondences for all time) (*1*). If we preserved the topology instead of probabilities, we would be studying the qualitative theory of ordinary differential equations initiated by Poincaré.

Abstract dynamical systems are natural objects from the mathematical point of view, and they arise in many different contexts (even in areas as far afield as number theory); elucidating their structure is considered an important mathematical problem. Much of the work in ergodic theory has had little to do with its initial motivation, but recently certain problems, some of which had been unsolved for more than a decade, have been solved, and a group of results has been obtained that does relate to concrete systems such as the billiard system. These results, which I will refer to as isomorphism theory, center around a better understanding of a certain abstract dynamical system called the baker's transformation. This is a map of the unit square onto itself (Fig. 1). We first stretch the square, doubling its width and halving its height. We next stack the right half of the elongated rectangle above the left half (the shape is again a square). These two steps give the baker's transformation. If looked at properly the baker's transformation can serve as the mathematical model for coin tossing (this is easy to see), and in this sense is completely random while it is deterministic in the sense that every point moves in a definite way.

The baker's transformation is often used as a paradigm for explaining the possibility of deterministic chaos (*2, 3*), that is, systems that evolve according to Newton's laws but nevertheless appear to be random. In recent years people have become increasingly aware of the ubiquity of this phenomenon.

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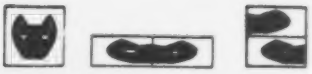

Fig. 1. The baker's transformation.

New results follow from isomorphism theory:

1) At the level of abstraction of isomorphism there is a unique (4, 5) system that is the most random possible. I will refer to it as the Bernoulli flow or B_1.

Even though "most random" is not well defined, I will explain some precise results that will make the above statement clear. It is important, however, to identify isomorphic systems and, unless we do this, "most random" has no meaning. Because there is no agreed definition of "chaotic" in the literature on chaos, result 1 gives us something definitive to aim at when studying specific systems.

2) If we look at B_0, in other words, a system isomorphic to B_1, at regular time intervals (for example, every hour on the hour), then this new system is isomorphic to the baker's transformation.

3) There are systems that obey Newton's laws that are isomorphic to B_1. A billiard system with one or several convex obstacles is an example (without the obstacle it is not chaotic). Other examples of B_1 are geodesic flow on a manifold of negative curvature and the Lorenz attractor. Dissipative examples of B_1 are provided by Smale's axiom A attractors. Isomorphism theory provides the only method so far for completely determining the statistical structure of specific chaotic systems, in other words, determining them up to isomorphism.

I conjecture that "most" chaotic systems that arise naturally are abstractly the same as B_1. This would mean that much of the diversity that we see arises from different ways of looking at the same abstract system.

Isomorphism in general can be very hard to visualize—very different systems can be isomorphic—but this is the price we have to pay for the generality of our results. On the other hand, in special cases the theory produces isomorphisms that preserve some of the geometry as well as the statistical structure. I will explain these results in terms of two examples, both of which are isomorphic to B_1.

The first is the motion of a billiard ball on a rectangular table with a convex obstacle. This example exhibits what people call deterministic chaos. The second example is the random jumping of a point on the same table, the jumps being determined by flipping a biased coin. This is generally thought of as truly "random."

In terms of these examples we have the following results:

4) If we perturb the obstacle slightly in our billiard system, we get essentially the same collection of infinite trajectories. More precisely, the system and its perturbation are isomorphic and corresponding points are close, with high probability. This means that we cannot tell if we are looking at our original system through a "viewer" (6) that distorts slightly or at our perturbed system (through a viewer that does not distort at all), even if we look for all time.

This result is surprising because effects of perturbing the obstacle are cumulative whereas the distortions produced by a viewer are not (the viewer does not interfere with the orbit, it only misreads it slightly).

5) If we design the random system carefully (still using a biased coin), then this random system and the billiard system will have essentially the same trajectories even in the long run (the short-term approximation of trajectories is not hard). One can even reproduce the random system exactly by observing the billiard system through a specially constructed viewer of finite precision that does not distort very much. Note that the viewer, as before, is not random, and thus our random system could be produced by Newton's laws or by coin flipping.

This article has a dual purpose. I would like to explain some results in ergodic theory by describing what they say in a simple concrete context. I also hope that these results will shed some light on the nature of chaos. For a more complete technical version of this article, see (7), which is based largely on (8).

The Phase Space of a Dynamical System

Consider the example of a billiard ball moving on a rectangular table with a convex obstacle. The ball moves in a straight line at unit speed and bounces off the obstacle and the edges with the angle of reflection equal to the angle of incidence.

The configuration of the system is completely determined by the position and velocity of the ball. The phase space is the set of all configurations of the system. Each point (configuration) in the phase space moves along a well-defined trajectory or orbit in the phase space.

Phase space also has a probability structure: every reasonable (in other words, measurable) set in phase space is assigned a probability—the probability of finding the system in that set. The probability structure has the property that, as a set evolves in time, its probability does not change.

Isomorphism and Abstract Dynamical Systems

Definition: Two systems are isomorphic if (after we ignore sets of probability zero) there is a one-to-one correspondence between the points of their phase spaces, where corresponding sets have the same probability and orbits correspond to orbits in a time-preserving manner.

Isomorphism means "the same" relative to the structure we are interested in (here, the statistical dynamical structure) and abstracts out everything but this structure. We could think of an abstract system as an equivalence class under isomorphism or, more directly:

Definition: An abstract system is a set of points that play the role of the phase space, in which each reasonable (Lebesgue measurable) subset has a probability (that does not change as the subset moves) and a rule that tells us where a point will move in t units of time (9). Isomorphic systems can be thought of as being the same abstract system (10).

An application of isomorphism theory is that all our billiard systems with obstacles are isomorphic to each other and to B_1 but not to the one with no obstacles.

The classification of chaotic dynamical systems by their abstract system ran into difficulties because the abstract system could not be determined in any specific case. However, isomorphism theory gives criteria for determining if a specific dynamical system is isomorphic to B_1. This gives, essentially, the only cases of chaotic systems for which the abstract dynamical system associated with a specific system can be determined exactly.

Stability

Individual orbits of chaotic systems can be extremely unstable and nonreproducible. On the other hand, for many chaotic systems, we can prove that the system as a whole is stable. We will describe our result in the case of billiard balls with convex obstacles.

Theorem 1. Pick a small $1 \gg \alpha > 0$. Suppose we change the position or curvature of the obstacle by a small amount (how small

depends on α). Then there is (after we ignore sets of probability zero) a one-to-one correspondence between points of the phase space so that, with probability $1 - \alpha$, corresponding points are closer than α, corresponding sets have the same probability, and orbits of the original system correspond to orbits of the changed system in a time-preserving manner. [To be completely accurate we may need a small rescaling of time, $t \to \alpha t$, $1 - \alpha < \alpha$, in one of the systems (5).]

Let me rephrase this in a more picturesque way. We can think of the above correspondence as being implemented by looking at our system through a viewer. In other words, when we look at the ball through the viewer, we see a slightly distorted position and velocity, the one that corresponds (via the above correspondence) to the position and velocity we are looking at. The viewer thus distorts each orbit by misreading it slightly. Our result says that the perturbed system is identical to the original system seen through our viewer, which distorts only slightly. (We get the same collection of orbits, and all joint probabilities are the same.)

The interest in this result lies in the fact that changes introduced by changing the obstacles are cumulative, their effect builds up as time goes on, whereas the changes introduced by the viewer clearly do not build up. The viewer does not interfere with the orbit, it only misreads it slightly.

Theorem 1 is delicate. For example, no matter how closely we approximate the obstacle by a polygon (a large change in curvature), any orbit of the original system and any orbit of the changed system will be D apart on average, where D is the average distance between points in the phase space (even if we rescale time). This means that, if we cannot distinguish points that are too close, we can change the system by an amount that is too small to see but still be able to observe the effect by looking at the system for a long time.

Our results on stability are analogous to the structural stability results of Anosov and Smale. Both kinds of stability hold for smooth perturbations of axiom A systems. (These systems include most of the examples that can be analyzed rigorously.) The main differences are as follows:

1) Our correspondence between phase spaces (the one in theorem 1) preserves probability whereas that of Anosov and Smale preserves the topology (homeomorphism). Note that a homeomorphism can take a set of probability 1 to a set of probability zero.

2) The structural stability correspondence moves all points by a small amount. Our correspondence moves most (except for a set of small probability) points by only a small amount.

3) Our correspondence takes orbits to orbits with a constant rescaling of time ($t \to \alpha t$, $\alpha \approx 1$). In structural stability the rescaling may differ from point to point (so corresponding sets may not evolve in the same way).

Random Systems and Their Phase Space

Following Kolmogorov, I will now introduce the analog of phase space for random processes. This will cast random processes and Newtonian systems in the same mathematical framework and allow for rigorous comparison.

Let us do this for coin tossing. We describe the outcome of each coin tossing experiment (the experiment goes on for all time) as a doubly infinite sequence of heads (H's) and tails (T's). The phase space is the collection of these sequences. Each subset is assigned a probability. For example, the set of all sequences that are H at time 3 and T at time 5 gets probability 1/4. The passage of time shifts each sequence to the left (what used to be time 1 is now time 0).

Definition: The dynamical system or transformation I have just described is called the Bernoulli shift $B_{(p_1,p_2)}$ and may be identified with the baker's transformation. If instead of flipping a coin, we spin a roulette wheel with three slots of probability p_1, p_2, p_3, we would get the Bernoulli shift $B(p_1,p_2,p_3)$.

We identify $B_{(\frac{1}{2},\frac{1}{2})}$ with the baker's transformation as follows. We let H,T correspond to 0,1 and split the two-sided infinite sequences into two one-sided infinite sequences such as $\ldots 1000.1101 \sim 0.0001\ldots, 0.1101\ldots$. These one-sided sequences give the dyadic expansion of the x and y coordinates of a point in the square. The reader can check that under the correspondence I have just given between sequences of H,T and points in the square, coin tossing probabilities correspond to ordinary area and that the shift on sequences H,T corresponds to the baker's transformation defined in the introduction.

The random process described in the introduction, called a semi-Markov process, gives rise to a dynamical system in the same way. The process can be defined as follows. We stay at one of a finite number of points x_i on the billiard table for time t_i and then jump to one of a pair of points according to a flip of a biased coin (the pair of points depends on x_i). It can be shown that this new dynamical system is isomorphic to $B_{\frac{1}{2}}$.

We can think of the dynamical system that we get from a random process as an abstract model for the minimal mechanism capable of producing that process. If we start with a dynamical system and make a measurement (which can be modeled as a function on the phase space), then we can think of the outcome of the measurement at various times as a sample path of a stationary process.

The phase space model for a random process means, in some sense, that every random process could be produced by a deterministic mechanism. These mechanisms, however, are not governed by Newton's laws. A much harder problem, mathematically, is to show that the same system can arise both from Newton's laws and from processes based on coin flipping.

Random Versus Deterministic Systems

I will give an example of how the randomness of systems isomorphic to $B_{\frac{1}{2}}$ manifests itself in a more concrete way by comparing a system governed by Newton's laws, our billiard system, with a random system based on coin flipping, our semi-Markov system.

Theorem 2. There exists, in theory, a nonrandom viewer that is as reliable as we want, and the viewer has the property that the billiard system with a convex obstacle seen through this viewer is identical (all joint probabilities are the same) to some semi-Markov system. Furthermore, we can still completely reconstruct the orbit we are looking at from the random orbit that we see.

Technically the viewer is a measurable function from the phase space to the finite collection of points on the table. It is reliable in the sense that for most points in the phase space (position and velocity pairs) the position of the ball we are looking at is close to the position of the special point that we see. This viewer models finite precision measurements.

In the billiard system, we can get as much information as we want about the state at time zero by making arbitrarily fine observations at time zero. The analogy of this with the viewer is that we make regular observations of fixed accuracy and we get as much information as we want about the state at time zero by making the sequence of observations go longer into the past and into the future.

Theorem 2 says that the semi-Markov system can be thought of as being produced by Newton's laws. Theorem 2 holds for all systems isomorphic to $B_{\frac{1}{2}}$. A sense in which $B_{\frac{1}{2}}$ is "the most random" is that theorem 2 only holds for systems isomorphic to $B_{\frac{1}{2}}$.

A common belief is that the appearance of randomness comes

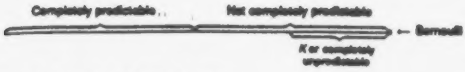

Fig. 2. The kinds of chaotic behavior.

Table 1. Predictability of finite precision observations at discrete time intervals (every hour on the hour).

Class of flow	Holds for	Knowledge of the past tells us
Completely predictable	All observations	Everything about the future
Not completely predictable	Some observations	Not everything about the future
K	All observations	Arbitrarily little about fixed time in distant future
Bernoulli	All observations	Vanishingly little about the long-term future

from some microscopic random phenomenon, such as thermal agitation, which is magnified because of sensitivity to initial conditions. Theorem 2 says that we can get random-looking behavior without assuming any underlying randomness. However, we can still ask about the effect of adding some small underlying randomness. It can be shown that in certain cases we cannot distinguish between randomness in the system and randomness in the device through which we view the system. The system that results from adding a small amount of randomness could be reproduced exactly (all joint probabilities are the same) if we were to look at the original system through a viewer that distorts randomly but not very much (with high probability). Such a viewer does not interfere with the process and its effect is not cumulative, whereas the effects of the original perturbation do accumulate, making this result somewhat surprising. [An exact statement and some cases where this holds are given in (7).]

Bernoulli Flow B_1, the Most Random Flow Possible

Theorem 3. There is an abstract system that we call B_1 with the property that if we only look at it on the hour, then it is isomorphic to the Bernoulli shift, $B_{(1/2,1/2)}$. If we were to look at B_1 on the minute instead of on the hour, we would get another Bernoulli shift; in fact, we get all Bernoulli shifts in this way. Any flow for which we can get a Bernoulli shift by discretizing time is isomorphic to B_1 after an appropriate rescaling of time (*5*). Thus B_1 is "most random" in the sense that independent processes are the most random discrete time processes.

A more definitive justification for calling B_1 "most random" comes from examining the predictability of measurements on a system isomorphic to B_1. Recall that a measurement gives rise to a stationary process. We will say that measurements (with the same range) on different systems give rise to the same process if all joint probabilities are the same.

Before discussing B_1, I should point out that there is a class of systems that are the least random—where all measurements (even those of finite precision) are predictable in the sense that, if made at regular intervals of time, the past determines the future. We call these systems completely predictable.

Isomorphism theory shows:

Theorem 4. If a system has any observation that is not predictable, then the set of processes arising from observations on the system includes all the processes that can arise from observations on B_1. Furthermore, if a system is not isomorphic to B_1, then it gives rise to some measurement that is more predictable (in a sense discussed in the next section) than any measurement on B_1.

The following theorem, which is related to theorem 4, shows that B_1 is present, in some sense, in all chaotic systems and that it has a unique place among these systems.

Theorem 5. Any system that is not completely predictable has B_1 [with a possible rescaling (*5, 11*) of time] as a factor. Furthermore, the only factors of B_1 are B_1 (with a rescaling of time that slows the speed of B_1).

System a is a factor of system b if there is a many-to-one correspondence from the phase space of b to that of a, where corresponding sets have the same probability and evolve in the same way. In other words, we can make b isomorphic to a by lumping points together and treating these lumps as single points. Lumps must, of course, go into lumps under time evolution.

One of the most important features of isomorphism theory is that it allows one to show that specific systems are isomorphic to B_1. We have already encountered several such examples. A result of Pesin says that, when the phase space is three-dimensional, then essentially the only possibilities are completely predictable or B_1.

Theorem 6. Suppose we have a Hamiltonian system with a three-dimensional phase space, after fixing the energy. The phase space will then break up into possibly one but at most a countable number of invariant sets with the property that on each of these the system is either completely predictable or [after rescaling time (*5*)] isomorphic to B_1 or isomorphic to a minor modification of B_1 (*12*).

No one has been able to rigorously analyze the double pendulum (a pendulum with a joint in the middle) but Pesin's theorem says that it is either the most random possible or completely predictable. Computer studies make it highly unlikely that it is completely predictable.

The Varieties of Chaotic Behavior

At one extreme we have a single flow B_1, whereas at the other extreme we have the class of flows that are completely predictable (the billiard system with no obstacles is an example). The flows that are not completely predictable are generally considered chaotic. Not being completely predictable is essentially the same as having sensitivity to initial conditions, positive Lyapunov exponents, or positive entropy.

A flow that is not completely predictable may have some predictable and some nonpredictable measurements. There is a subclass that is especially chaotic, the so-called *K* flows, where no measurement is predictable.

It was once hoped that the general picture would be fairly simple, at least in discrete time. It had been conjectured that the only discrete-time *K* systems were the Bernoulli shifts and that every discrete system was the product of a *K* system and a completely predictable system (the Pinsker conjecture). The latter would mean that every stationary process could be realized as a function of the joint output of a completely predictable system and a *K* system (the two systems not interacting).

Theorem 7. There exist systems that are not the product of a completely predictable system and a completely nonpredictable system.

Theorem 8. There are *K* transformations that are not Bernoulli shifts and *K* flows that are not B_1 (Fig. 2).

There are many theorems to the effect that the *K* class is extremely complicated. In particular, the fact that so many different systems are isomorphic to B_1 is not due to a lack of possible alternatives. These examples also show that there are chaotic processes that cannot be

closely simulated by more general semi-Markov processes (for example, if we jump to more than two points and replace coin flipping with a more complicated independent process). Thus, not all randomness can be understood in terms of coin flipping or independent processes (7).

We can also characterize the different degrees of randomness by the predictability of observation made on the systems (Table 1). I will not state these results precisely but refer the reader to (7) for the precise statement; Table 1 should give the reader a rough idea.

Dissipative Systems

The main example I used to illustrate our results was billiards, a conservative system. These results, however, apply to a large class of dissipative systems, those that satisfy axiom A. In this situation there is an invariant set in the phase space, the attractor. The attractor has an invariant probability structure and is exactly the kind of dynamical system I have been describing (it is even isomorphic to B_2). The difference is that the probability of our system being on the attractor is zero. However, with probability 1, every trajectory of our system is asymptotic to a generic trajectory on the attractor, and thus the dynamics of the attractor still governs the observed long-term behavior of the system.

I conjecture that the above situation is typical of dissipative systems. In particular, I conjecture that long-term statistical averages exist for dissipative as well as conservative systems.

Historical Overview of Isomorphism Theory for Chaotic Systems

Isomorphism theory for chaotic systems was initiated in 1958 when Kolmogorov and Sinai (13) introduced the concept of entropy to ergodic theory and used it to solve a long-standing problem by showing that not all Bernoulli shifts were isomorphic. [They showed that the entropy of $B(p_1,p_2,p_3)$ was $p_1 \log p_1 + p_2 \log p_2 + p_3 \log p_3$, and so forth, and that shifts of different entropy could not be isomorphic (13).] Sinai showed that a Bernoulli shift was a factor of anything not completely predictable (in discrete time) (14).

On the concrete side, Adler and Weiss proved an isomorphism theorem for automorphisms of the 2-torus (15), and Sinai (16) and Anosov (17) showed that a large class of systems (including billiard balls with obstacles) were completely nonpredictable. Anosov also showed that these systems were stable in a topological rather than a statistical sense (structural stability) (17).

The first of the group of results discussed in this article came in 1970 when Ornstein showed that Bernoulli shifts of the same entropy were isomorphic (18) by methods different from those of Sinai and those of Adler and Weiss. By a still different set of ideas Ornstein showed that the completely unpredictable class contained more than the Bernoulli shifts (19) and that not every transformation was the direct product of a completely unpredictable and a completely predictable transformation (20).

With the method used to prove the isomorphism theorem for Bernoulli shifts, Ornstein showed that Bernoulli shifts could arise in the context of real (continuous) time, that there was a unique Bernoulli flow (which strings together all of the Bernoulli shifts), that the only factors of B_t are B_s, and that B_t is a factor of any system that is not completely predictable (21).

The connection with concrete systems was made when Weiss and Ornstein showed that the geodesic flow on a surface of negative curvature is isomorphic to B_t (22). Since then it has been shown that a large class of specific flows were isomorphic to B_t. This was done by using the work of Sinai, Anosov, Pesin, and others (23) to check a criterion (8) that makes the isomorphism theorem work.

In many cases the isomorphisms produced by the theory do not move points very much and we get theorem 1, a statistical version of Anosov's results on structural stability, and theorem 2 (7).

There is a series of results (24), due mainly to Feldman, Rudolph, Thouvenot, Weiss, and Ornstein, that is a continuation of the abstract results mentioned above and that contains some of the deepest results in this subject. Unfortunately I do not have the space to describe them.

There is also an area of finitary codes that are a continuation of the result of Adler and Weiss but are less directly related to the results of this article. The ones most closely related are (25).

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1. I would emphasize that we are discussing statistical properties and ignoring events of probability zero, even in concrete examples. For example, embedded Smale horseshoes (a topological version of the baker's transformation) typically have probability zero (when a probability structure exists) and so lie outside the scope of ergodic theory.
2. I. Ekeland, *Mathematics and the Unexpected* (Univ. of Chicago Press, Chicago, 1988) (Ekeland calls the baker's transformation Arnold's cat).
3. I. Prigogine, *From Being to Becoming* (Freeman, San Francisco, 1980), appendix A and p. 187.
4. Technically, B_t and B_s after a rescaling of time (for example, doubling the speed of the flow) are not isomorphic. To be absolutely precise we need to say that B_t is unique up to rescaling of time.
5. When I rescale in this article, the speed change will not be different at different points, as it can be in structural stability theory, because the latter change could affect the way that sets evolve and so have a drastic effect on the qualitative behavior of the system.
6. Technically the viewer is just a measurable function; in this case it is the correspondence that gives our isomorphism.
7. D. S. Ornstein and B. Weiss, *Am. Math. Soc. Bull.*, in press.
8. D. S. Ornstein, *Ergodic Theory, Randomness, and Dynamical Systems* (Yale Mathematical Monograph 5, Yale Univ. Press, New Haven, 1974).
9. Abstract systems can retain a lot of structure. For example, every surface of negative curvature carries an abstract system, the horocycle flow, from which the surface can be completely reconstructed.
10. The information that we have lost in this abstraction is contained in a function on the phase space, in the case of the billiard system the function that identifies a point in the abstract phase space with a particular position and velocity.
11. The rescalings of time are those rescalings that make the entropy of B_t less than or equal to the entropy of the first system.
12. The modification of B_t is the product of B_t and a rotation of the circle. Such a system can be thought of as taking a system isomorphic to B_t together with a watch. A configuration of this system consists of a configuration of the old system and the time of day. Pesin's theorem is more general, holding for any smooth flow on a three-dimensional compact manifold that preserves a smooth measure.
13. Ja. G. Sinai, *Dokl. Akad. Nauk SSSR* **124**, 768 (1959); A. N. Kolmogorov, *ibid.* **119**, 861 (1958); *ibid.* **124**, 754 (1959).
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15. R. Adler and B. Weiss, *Am. Math. Soc. Mem.* **98** (1970).
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18. D. S. Ornstein, *Adv. Math.* **4**, 337 (1970). The following books are devoted mainly to an exposition of this theorem: P. Shields, *The Theory of Bernoulli Shifts* (Univ. of Chicago Press, Chicago, 1973); M. Smorodinsky, *Ergodic Theory, Entropy* (Springer Lecture Notes 214, Springer, New York, 1970).
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20. D. S. Ornstein, *ibid.*, p. 89; *ibid.*, p. 103.
21. ———, *ibid.* **5**, 349 (1970); in *Contributions to Ergodic Theory and Probability* (Lecture Notes in Mathematics, Springer-Verlag, Berlin, 1970), pp. 178–218; *Adv. Math.* **10**, 124 (1973).
22. B. Weiss and D. S. Ornstein, *Isr. J. Math.* **14**, 184 (1973).
23. Billiard balls are treated in G. Gallavotti and D. S. Ornstein, *Commun. Math. Phys.* **38**, 83 (1974). Axiom A and the Lorenz attractor are treated in M. Ratner, *Isr. J. Math.* **17**, 380 (1974), and L. Bunimovic, *Sov. Math. Izv.* **8**, 219 (1974). Pesin's theorem can be found in Ya. Pesin, *Russ. Math. Surv.* **32** (no. 4), 55 (1977).
24. Entropy theory is developed for continuous time (without reducing to discrete time) in J. Feldman, *Isr. J. Math.* **36**, 321 (1980). The finite fiber factors of a Bernoulli shift are classified in D. Rudolph, *J. Anal. Math.* **34**, 36 (1978). A theory parallel to isomorphism theory for nonconstant speed changes is developed in J. Feldman, *Isr. J. Math.* **26**, 16 (1976); D. Ornstein, D. Rudolph, B. Weiss, *Am. Math. Soc. Mem.* **37** (no. 262) (1982); D. Rudolph, *ibid.* **54** (no. 323) (1985). Isomorphism theory for amenable group actions is developed in D. Ornstein and B. Weiss, *J. Anal. Math.* **48**, 1 (1987). Isomorphism theory is developed for

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